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CONDUCTED BY

SIR DAVID BREWSTER, K.H. LL.D. F.R.S.L. & E. &c
SIR ROBERT KANE, M.D., F.R.S., M.R.I.A.
WILLIAM FRANCIS, PH.D. F.L.S. F.R.A.S. F.C.S.
JOHN TYNDALL, F.R.S. &c.

“Nec araneorum sane textus ideo melior quia ex se fila gignunt, nec noster
vilior quia ex alienis libamus ut apes.” JUST. LIPS. *Polit.* lib. i. cap. 1. Not.

VOL. XXIV.—FOURTH SERIES.

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“Meditationis est perscrutari occulta; contemplationis est admirari perspicua Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem.”—*Hugo de S. Victore.*

—“Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condât,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu.”

J. B. Pinelli ad Mazonium.

CONTENTS OF VOL. XXIV.

(FOURTH SERIES.)

NUMBER CLVIII.—JULY 1862.

	Page
M. A. J. Ångström on the Fraunhofer-lines visible in the Solar Spectrum, (With a Plate.)	1
Mr. H. F. Baxter on Nerve Force.—Relation of Nerve Force to Electric Force. Origin of Nerve Force.	11
Mr. A. Cayley on the Transcendent $gd . u = \frac{1}{i} \log \tan (\frac{1}{4}\pi + \frac{1}{2}ui)$	19
Dr. Atkinson's Chemical Notices from Foreign Journals	22
Drs. A. Matthiessen and C. Vogt's Reply to Mr. R. Sabine's "Remarks on the Influence of Traces of Foreign Metals on the Electric Conducting Power of Mercury"	30
Mr. J. Cockle on a New Species of Differential Equations.	37
The Rev. W. Roberts on some Applications of a Theorem relating to Parallel Surfaces	39
Prof. Bunsen on the Preparation of the Rubidium Compounds.	46
Prof. Potter's Observations upon a Paper by M. De la Rive "On the Auroræ Boreales"	51
Notices respecting New Books:—Prof. Kirchhoff's Researches on the Solar Spectrum	52
Proceedings of the Royal Institution:—	
Prof. Tyndall on Force	57
Proceedings of the Royal Society:—	
Mr. J. A. Broun on the Lunar Semidiurnal Variation of the Barometer; and on the Law of Disturbance and the Range of the Diurnal Variation of Magnetic Declination near the Magnetic Equator	66
Dr. J. H. Gladstone on the Atmospheric Lines of the Solar Spectrum, and on certain Spectra of Gases	70
Proceedings of the Geological Society:—	
Prof. Harkness on the Position of the <i>Pteraspis</i> -beds	73
Mr. W. Whitaker on the London Basin	74
Mr. J. Bolton on a Clay-deposit with Insects, Leaves, &c. near Ulverston	74
Prof. Huxley on two new Labyrinthodonts	75
Dr. Dawson on the Land Flora of the Devonian Period in North-eastern America	75
Prof. Sandberger on some Upper Eocene Fossils.	76
On the Electromotive Force of Voltaic Piles, by M. Marie Davy.	76
Examination of the Window-glass of Pompeii, by M.G. Bontemps.	78
On a Question in the Theory of Probabilities, by Prof. Boole.	80

NUMBER CLIX.—AUGUST.

Prof. Clausius on the Application of the Theorem of the Equivalence of Transformations to the Internal Work of a mass of Matter	81
Major-General Sabine on the Cosmical Features of Terrestrial Magnetism.....	97
Dr. Joule on the History of the Dynamical Theory of Heat ..	121
Prof. Field on some of the Basic Salts of Copper	123
MM. J. G. S. van Breda and W. M. Logeman on Ampèrian Repulsion	126
Sir W. R. Hamilton on the Existence of a Symbolic and Bi-quadratic Equation, which is satisfied by the Symbol of Linear or Distributive Operation on a Quaternion	127
Prof. Plateau on the Figures of Equilibrium of a Liquid Mass without Weight.—Sixth Series	128
Prof. Challis on the Mathematical Theory of the Vibrations of an Elastic Fluid.....	135
Proceedings of the Royal Society:—	
Prof. W. Thomson on the Measurement of Electric Resistance	149
Proceedings of the Royal Institution:—	
Prof. Faraday on Gas-Furnaces, &c.....	162
Proceedings of the Geological Society:—	
Prof. Harkness on the Metamorphic Rocks of the Banffshire Coast, &c.....	165
The Rev. D. Honeyman on the Geology of the Gold-fields of Nova Scotia	165
Mr. J. W. Salter on some Fossil Crustacea; on some Species of <i>Eurypterus</i> and allied forms; on <i>Peltocaris</i> ; and on a Crustacean Track in the Llandeilo Flags of Shropshire	166
On the Density of Ice, by L. Dufour	167
On a new Metal in the Native Platinum of Rogue River, Oregon, by C. F. Chandler, Professor of Chemistry at Union College.	168

NUMBER CLX.—SEPTEMBER.

Prof. Tyndall on the Conformation of the Alps	169
Prof. Tyndall on Mayer, and the Mechanical Theory of Heat..	173
M. R. Franz on the Diathermancy of the Media of the Eye....	176
Dr. Atkinson's Chemical Notices from Foreign Journals	185
Mr. G. B. Jerrard's Note on some Objections of Mr. Cayley and Mr. Cockle	193
Prof. Challis on the Principle of Discontinuity in Solutions of Problems in the Calculus of Variations.....	196
Prof. Clausius on the Application of the Theorem of the Equivalence of Transformations to the Internal Work of a mass of Matter	201

	Page
Prof. Regnault on the determination of Temperature and Pressure in Balloon Ascents. (With a Plate.)	213
M. W. Beetz on the Colour of Water. (With a Plate.)	218
Proceedings of the Royal Society :—	
Mr. J. P. Gassiot on the Heat which is developed at the Poles of a Voltaic Battery during the Passage of Luminous Discharges in Air and in Vacuo	225
Mr. R. Mallet on the Transit-velocity of Earthquake-waves.	229
Dr. Frankland on the Influence of Atmospheric Pressure upon some of the Phenomena of Combustion	232
The Earl of Rosse on the Nebulæ.	237
Mr. T. Graham on Liquid Transpiration in relation to Chemical Composition	238
Proceedings of the Geological Society :—	
Dr. Falconer on the disputed affinities of <i>Plagiaulax</i>	240
Prof. O. Heer on Fossil Plants from the Isle of Wight ..	241
Mr. G. H. Morton on Glacial Surface-markings on the Sandstone near Liverpool	241
On Ground-Ice in Rivers, by Prof. J. Thomson	241
On the Temperature of the Inferior Layers of the Air, by M. Becquerel.	244
On the Refractive Indices of Bodies which only assume the Gaseous Condition at High Temperatures, by M. F.-P. Leroux	245
On the Calm Lines often seen on a Rippled Sea, by Professor James Thomson.	247

NUMBER CLXI.—OCTOBER.

Prof. Rijke on some Properties of the Induced Current	249
Prof. Potter's Mathematical Discussion of the properties of the Air Thermometer with respect to its fitness for determining the instantaneous changes of Temperature of its contained Air, arising from sudden changes of Density	263
Prof. Tyndall on the Absorption and Radiation of Heat by Gaseous Matter.—Second Memoir	270
Mr. J. Cockle on Symbolical Decomposition; and on the last two Papers of Mr. Jerrard	288
Mr. A. Cayley's Final Remarks on Mr. Jerrard's Theory of Equations of the Fifth Order.	290
Prof. Challis on the Hydrodynamical Theory of Vibrations. . . .	291
Mr. B. Stewart on the Nature of those Red Protuberances which are seen on the Sun's Limb during a Total Eclipse.	302
Dr. Atkinson's Chemical Notices from Foreign Journals	305
Proceedings of the Royal Society :—	
Prof. Matteucci on the Secondary Electromotor Power of Nerves	311
Mr. B. Stewart on the Great Magnetic Disturbance of August 28 to September 7, 1859	315

	Page
Dr. Matthiessen and M. von Bose on the Lead-Zinc and Bismuth-Zinc Alloys; and on some Gold-Tin Alloys ..	317
Proceedings of the Geological Society:—	
Prof. Jukes on the Mode of Formation of some of the River-valleys in the South of Ireland	323
Prof. Haughton on the Granites of Donegal	323
Prof. Huxley on a Stalk-eyed Crustacean from the Coal-measures; on the Premolar Teeth of <i>Diprotodon</i> , and on a New Species of that Genus from Australia	323
Mr. J. Powrie on the Old Red Sandstone of Fifeshire ..	324
Mr. E. W. Binney on some Upper Coal-measures, containing a bed of Limestone, at Catrine in Ayrshire ..	324
Prof. Nicol on the Structure of the Southern Grampians ..	324
Mr. S. H. Beckles on some Natural Casts of Foot-prints.	325
Mr. R. Thornton's Geological Notes on Zanzibar	325
Mr. W. Carruthers on a Section at Junction-road, Leith ..	325
Sir W. Denison on the Death of Fishes in the Sea during the Monsoon	326
On Ampèrian Repulsion, by James Croll	326
On the Changes in the apparent Size of the Moon	326
On the Dynamic Theory of Heat, by George F. Rodwell	327
On the Reduction of Chromium from Solution of the Chloride, by Charles W. Vincent	328
Reduction of Manganese from the Protochloride by Sodium-amalgam, by W. B. Giles	328

NUMBER CLXII.—NOVEMBER.

M. Edlund on the Thermal Phenomena which accompany the changes in volume of Solid Bodies, and on the corresponding Mechanical Work	329
Prof. Tyndall on the Absorption and Radiation of Heat by Gaseous Matter.—Second Memoir	337
Prof. Lamont on Dalton's Theory of Vapour, and its Application to the Aqueous Vapour of the Atmosphere	350
Mr. C. Tomlinson on the Motion of Camphor towards the Light	358
M. J. R. Mayer on the Forces of Inorganic Nature	371
Mr. A. C. Ramsay on the Excavation of the Valleys of the Alps.	377
Mr. D. Campbell on the Action of Nitric Acid upon Pyrophosphate of Magnesia	380
Dr. Atkinson's Chemical Notices from Foreign Journals	381
Mr. T. Zeno on the Changes in the Apparent Size of the Moon.	390
Proceedings of the Royal Society:—	
Prof. B. C. Brodie on the Oxidation and Disoxidation effected by the Peroxide of Hydrogen	392
Prof. J. Thomson on Crystallization and Liquefaction, as influenced by Stresses tending to change of form in Crystals	395

	Page
Mr. G. Gore on the Production of Vibrations and Sounds by Electrolysis; and on the Nature and Qualities of Voltaic Currents	401
Dr. Matthiessen and M. von Bose on the Influence of Temperature on the Electric Conducting Power of the Metals	405
On the Combustion of Gunpowder <i>in vacuo</i> , and in various Gaseous Media, by M. Bianchi	407
Observations on the Solar Spectrum, by Dr. A. Weiss	407
Mode of applying the Electric Light for Mining Purposes, by MM. Dumas and Benoit	408

NUMBER CLXIII.—DECEMBER.

Archdeacon Pratt's Tests of the Truth of the Fluid Theory of the Figure of the Earth	409
Dr. J. H. Gladstone on the Violet Flame of many Chlorides..	417
Dr. Rankine on the Exact Form and Motion of Waves at and near the Surface of Deep Water	420
Prof. Tyndall on the Absorption and Radiation of Heat by Gaseous Matter.—Second Memoir	422
Prof. Sylvester on the Integral of the general Equation in Differences	436
Sir David Brewster on the Action of various Coloured Bodies on the Spectrum	441
Prof. Potter on the Definition of the Temperature of Bodies, and on its Measurement by Thermometers	447
Mr. G. B. Jerrard on some Objections of Mr. Cayley and Mr. Cockle.	457
Prof. Challis's Explanations of Phenomena of Light on the Hypothesis of Undulations	462
Proceedings of the Royal Society:—	
Prof. Stokes on Internal Radiation	474
Prof. Stokes on the Intensity of the Light reflected from or transmitted through a Pile of Plates.	480
Mr. C. Tomlinson on the Motions of Camphor on the Surface of Water.	490
Proceedings of the Geological Society:—	
Dr. L. de Koninck on some Fossils from India	491
Miss E. Hodgson on a Deposit containing Diatomaceæ, &c., in the Iron-ore Mines near Ulverston	492
Capt. F. Applegath on the Geology of a part of the Masulipatam District	492
Mr. J. G. Sawkins on the Association of Granite with the Tertiary Strata near Kingston, Jamaica	492
On the Modifications which the Velocity of Light experiences in Glass, &c. under the influence of Heat, by M. H. Fizeau .	492
On Magnetizing Steel Needles by the Current of a Leyden Jar, by Dr. Paalzow	494
Note on the Limit of the Charge of Condensers, by M. Gauguain.	495

NUMBER CLXIV.—SUPPLEMENT.

Prof. von Kobell on Asterism and Brewster's Luminous Figures. (With Three Plates.)	497
Archdeacon Pratt's Geometrical Proof of the fundamental principle of Laplace's Functions	504
Archdeacon Pratt's Postscript to a Paper "On Tests of the Truth of the Fluid Theory of the Figure of the Earth"	507
Lieut.-Colonel A. Strange on Aluminium Bronze as a Material for the Construction of Philosophical Instruments	508
Mr. A. Cayley on the Skew Surface of the Third Order	514
Dr. Atkinson's Chemical Notices from Foreign Journals.	519
Mr. J. Cockle on the Correlations of Analysis.	531
Mr. R. P. Greg on some Meteorites in the British Museum, &c. .	534
Proceedings of the Royal Society:—	
Major-General Sabine on the Photographic Records of the Kew Declinometer in the years 1858–61	542
Dr. Hofmann on the Colouring Matters from Aniline. . . .	547
Dr. Phillips on a Systematic Representation of the Physical Aspect of the Moon	555
On a remarkably easy Mode of approximating to the Circumference of the Circle, by S. M. Drach, F.R.A.S., F.R.G.S. . .	560
On the Solar Energy as manifested in the autumn of 1861 and a portion of the spring of 1862, by W. R. Birt, Esq.	561

PLATES.

- I. Illustrative of M. A. J. Ångström's Paper on the Fraunhofer-lines visible in the Solar Spectrum; M. Beetz's Paper on the Colour of Water; and Prof. Regnault's Paper on the Determination of Temperatures and Pressures in Balloon-Ascents.
- II. III. IV. Illustrative of Prof. von Kobell's Paper on Asterism and Brewster's Luminous Figures.

ERRATA IN VOL. XXIII.

Page 448, for line 2 read

$$-\frac{3h}{2c^2}(\beta r^2 - (\beta - 1)t^2) + \frac{1}{2c^2}(\beta r^3 - (\beta - 1)t^3) \sqrt{\frac{2h}{c}}.$$

Page 459, line 14 from top, for 109·89 read 114·57.

— — — 15 — for 61·46 read 66·02.
 — — — 23, 38, — for results read result.
 — — — 31, — for 5·64 read 6·06.
 — 521, dele note *.

CORRIGENDA IN VOL. XXIV.

Page 38, line 13, for cb' read ac' .

— 39, — 2, for A read A_2 .

[Note. $A = aA_2$]

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
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[FOURTH SERIES.]

JULY 1862.

I. *On the Fraunhofer-lines visible in the Solar Spectrum.*

By A. J. ÅNGSTRÖM*.

[With a Plate.]

§ 1. **I**N a former memoir, "Optiska Undersökningar" (Optical Researches), published by the Royal Academy of Stockholm†, I have, for the purpose of illustrating the absorption of light, made use of a principle already propounded by Euler in his "Theoria lucis et caloris," viz. that the particles of a body, in consequence of resonance, absorb principally those ethereal undulatory motions which have previously been impressed upon them, and I extend the validity of this principle not only to the case in which the absorbed light displays itself sensibly as light and heat, but also to that in which its effect is evidenced by chemical decomposition. Conversely, I endeavour also to show *that a body in a state of glowing heat emits just the same kinds of light and heat which it absorbs under the same circumstances*; and it was in order to test the truth of this proposition, and for that purpose to ascertain the different kinds of light which different metals in a state of gaseous glow emit, that the above-mentioned examination of the electric-light spectrum was undertaken.

The results at which I arrived in the above-named disquisition were, in short, the following:—That the electrical spectrum, which had been the especial object of the researches of Fraunhofer, Wheatstone, and Masson, must be considered as the superposition of two spectra—the one belonging to the metal of which the electrodes are made, the other to the gas through

* Communicated by the Author, having been read before the Royal Academy of Science, Stockholm, Oct. 8, 1861.

† *Vet. Akad.*, Feb. 1853 (*Philosophical Magazine*, S. 4. vol. ix. p. 237).
Phil. Mag. S. 4. Vol. 24. No. 158. July 1862. B

which the spark passes ; also that, on account of the different appearance of the lines in the metallic spectrum, it is possible to distinguish it from that arising from the gas. This circumstance enabled me to construct figures of the spectra belonging to the metals most commonly met with, and to determine the relative position of the most prominent light-lines, with respect not only to the lines of the aerial spectrum, but also to the Fraunhofer-lines, with such accuracy as the means I could then command permitted. I also showed that metallic compounds and metallic sulphides have precisely the same lines in their light-spectra as each of the different bodies of which the compound consists, and that this circumstance offered a ready means of determining the constituent parts of an unknown substance. This circumstance by no means escaped my notice ; but for the metals with which I was then engaged the method appeared not sufficiently sensitive, and therefore inapplicable, although it has since, according to the experiments of Bunsen and Kirchhoff, shown a remarkable degree of sensitiveness in the case of the alkaline metals.

The results at which I arrived in that paper have since been for the most part confirmed by Willigen* in his excellent paper ; he, however, employed, as the source of his electricity, a Ruhmkorff's inductive apparatus, and made important changes in the method of observation by introducing between the two electrodes of the instrument chemical solutions, especially combinations of chlorine with the substances he wished to examine—a method which, in imitation of him, I have adopted in my later researches.

The Physical Cabinet at Upsala, having been last year provided with an optical theodolite by Pistor of Berlin, with an induction-apparatus by Ruhmkorff, with prisms and glass micrometer-scales by Steinheil, Bertand, and Mertz, I have resumed these researches with the advantage of Dr. Thalén's zealous assistance.

The object of these researches is twofold :—1st, a new examination of the solar spectrum, a determination of the lengths of the waves, not only for the seven principal lines already determined by Fraunhofer, but also for a sufficient number of intermediate lines, and the construction of accurate drawings of the spectrum, without which measurements can have no practical utility ; 2nd, the determination of the length of the waves for the various sorts of light which enter into the gas-spectra of different metals, a determination which is best effected by actually introducing these latter into the solar spectrum.

§ 2. The following notice of the Fraunhofer-lines, and of the conclusions to which they lead with respect to the composition

* Poggendorff's *Annalen*, vol. cvi. p. 610.

of the solar photosphere, must be considered as deductions from such portions of this research as have hitherto been completed.

I had already, in my former paper, remarked that the Fraunhofer-lines in the solar spectrum were, so to speak, an inversion of the bright lines in the electrical spectrum, and that an explanation of the lines in the one system would in all probability also furnish an explanation of those of the other, according to the principle laid down in that dissertation. I endeavoured to apply the often-named principle to the bright lines in the electrical *aërial* spectrum, and to the Fraunhofer-lines in the solar spectrum, though I had not then, nor have I since, found any more accurate correspondence between the two. I endeavoured to explain to myself the causes of this circumstance, principally by the differences of density and the different states of elasticity of the air during the passage of the solar light through the atmosphere, and at the moment of its being heated by the electric spark. This difference, however, cannot exist in the case of the gases which envelope the sun, and the presence of which is evidenced by the corona and the so-called protuberances visible during total eclipses of the sun.

It may now generally be taken for granted that the Fraunhofer-lines for the most part originate in the sun's photosphere*

* It has been urged as an objection to the hypothesis that the Fraunhofer-lines belong principally to the sun, that they ought in that case to appear stronger and more distinct when the rays come from the edge, than when they come from the centre of the disk, which, according to Forbes's observation, was not the case.

Forbes's observation took place under the different stages of a solar eclipse, under which circumstances I conceive it would be very difficult to preserve the appearance of the spectrum accurately in the memory. I therefore considered that experiment to be worth repeating.

I used for this purpose an optical theodolite with two telescopes, one of which was furnished with a slit-opening to admit the sunlight. The height of this orifice was reduced to about 3 millims., and the sun's image was projected upon it from a Dollond object-glass of 3.02 metres focal distance. The diameter of the image thus formed was 28 millims.; and by allowing the rays from different parts of this image to fall successively upon the opening, it was easy to see whether the Fraunhofer-lines underwent any change. Any very remarkable change I could not discover. All that I fancied I could remark was, that the intensity of the spectrum-light is somewhat less when the ray comes from the edge, than when from the centre of the disk; and this is evidenced by the circumstance that the fainter Fraunhofer-lines show themselves in the latter case comparatively stronger, whereas, when the light comes from the centre of the solar disk, the fainter lines will sometimes even totally disappear, while the stronger lines, as for example, some of the iron-lines, appear with correspondingly increased brilliancy. As we know from Kirchhoff's experiments that an increased difference of intensity between the source of light and the absorbing gas is favourable to the distinctness of the lines in the gas-spectrum, it would seem that this observation, if confirmed, is not repugnant to what we already know concerning the absorbing-power of gases.

or the gaseous envelope that immediately surrounds that body, even though some of these lines should belong to the atmosphere of the earth. As, moreover, the gases which immediately envelope the photosphere must be at a very high temperature, we are fully justified in applying the principle, that these gases *absorb just the same kinds of light which they emit in a state of glowing heat*. Accordingly by seeking out the bright lines in the electrical spectra of different metals which have corresponding dark lines in the solar spectrum, we can with considerable probability determine what metals in gaseous form enter into the composition of the solar envelope. I say only “with considerable probability;” for, from the circumstance of two lines coinciding in both the spectra of the sun and of a given metal, it by no means follows as a necessary consequence that this substance is to be found in the sun, because, on account of the enormous number of dark lines in the sun’s spectrum, such coincidence may be accidental; nevertheless the probability of such an assumption increases in proportion to the number of such coincident lines and their phenomenal peculiarities.

§ 3. Fraunhofer had already observed that the two soda-lines coincide with the double line D of the solar spectrum; and Brewster has made the interesting observation that the combustion of potassium gives lines which correspond with those of the extreme red end of the solar spectrum.

Kirchhoff, to whom belongs the honour of having first by direct experiments with sodium and lithium proved the truth of the law of correspondence between absorption and radiation, which I had laid down in the memoir so often referred to, has, in a memoir inserted in the *Philosophical Magazine*, and in the *Bibliothèque de Genève*, stated that he had found no less than sixty iron-lines having their corresponding lines in the solar spectrum between the lines F and D of the latter; moreover, that magnesium displays lines answering to the group *b*, and that nickel and chrome have corresponding lines in the sun’s spectrum. These statements we have verified and can confirm.

The number of iron-lines between F and D whose identity with the Fraunhofer-lines we can venture to guarantee, is not indeed so great as that affirmed by Kirchhoff, but it is quite sufficient to establish beyond a doubt the presence of iron in the solar atmosphere. The iron-lines are the most characteristic in the whole solar spectrum; and if a sufficient magnifying power be employed, or the light be refracted through two prisms, these lines, or at any rate the stronger ones among them, appear perfectly black.

The same remark, which I have already made with respect to the lines of the electrical spectrum, is in general true of the

Fraunhofer-lines; namely, that the lines belonging to the air or to the gas through which the electric spark passes, are less sharply defined and diffuse themselves more in proportion as the strength of the electricity is increased, whereas the metal-lines, especially those belonging to metals of high fusion-temperature, like iron, are very sharply defined.

On a careful examination of the solar spectrum, one can often discover certain lines imbedded as it were in a mass of fainter lines, which with increased illumination seem to withdraw themselves and disappear, while the first-mentioned lines, on the contrary, only stand out in stronger relief. These are metal-lines, and the most remarkable among them almost invariably belong to iron.

Thus between H and G we have fifteen stronger iron-lines, all having their counterparts in the solar spectrum. The two strongest of these lie at about $\frac{1}{4}$ and $\frac{3}{4}$ of the distance between H and G, and that nearest to G is double, as belonging also to calcium. Moreover, at G and in its immediate neighbourhood towards F, we have five strong besides eight fainter lines, at F, again, five, and, lastly, around E a numerous group, all answering to dark lines in the solar spectrum.

We may add to Kirchhoff's statement concerning magnesium, that the third of the lines marked *b*, reckoning towards F, is double, and belongs accordingly both to magnesium and iron.

To the four bodies which Kirchhoff has thus shown to exist in the sun, we may, as the result of our own researches, add calcium, aluminium, and manganese, as also in all probability strontium and barium.

Calcium has three strong lines at the violet end of the spectrum, of which two are especially remarkable as answering to the H lines, and the third in order forms, together with one of the iron-lines, the before-mentioned strong double line. Calcium has furthermore six lines coinciding with lines of the G group, three between G and F, and, lastly, groups of fine lines at E, and between E and G.

Aluminium is especially characterized by two strong lines lying between the H lines of the solar spectrum, and there corresponding to two dark lines. Between H and G, aluminium probably forms a continuous spectrum. The aluminium-lines in the remaining portion of the spectrum are less prominent; and accordingly, the corresponding lines of the solar spectrum cannot with certainty be determined.

The *manganese*-spectrum contains a considerable number of lines, which in their phenomena very much resemble those of iron. Between G and H, two groups of manganese-lines coincide with two similar groups of iron-lines; and between G and F are

found also thirteen manganese-lines closely approaching those of iron. The identity of most of these lines with corresponding lines of the solar spectrum is certain.

As regards *strontium* and *barium*, the former has between H and G two strong lines, which seem to correspond to lines in the sun's spectrum, and the same seems to be the case with the line F. At the same time we must add that the strong strontium-line that lies about midway between G and F, has no strong corresponding line in the solar spectrum, which circumstance makes the presence of strontium in the sun somewhat uncertain.

The *barium*-spectrum exhibits four lines between F and D, which seem to have their counterparts in the solar spectrum; but this also requires further confirmation.

Our examination of the red end of the spectrum, from D onwards, is less complete, and I accordingly forbear to speak of the lines that occur in that part of the spectrum. Moreover, the dispersion being here considerably less, the difficulty of determining whether two lines in the solar and metallic spectra respectively exactly coincide or not is proportionally increased.

When we sum up what has been adduced with respect to the *Fraunhofer-lines*, and assume that the line C belongs to hydrogen, we obtain the following general view of the lines in the spectrum:—

The two lines H belong to calcium.

The line G belongs to iron.

„	„	F	„	„	strontium and iron (uncertain).
„	„	<i>b</i>	„	„	magnesium and iron.
„	„	D	„	„	sodium.
„	„	C	„	„	hydrogen.
„	„	B	„	„	potassium.

As an example of our method of figuring the solar spectrum, as space does not permit us to insert the drawings in their integrity, we give in the accompanying Plate I. fig. 1, the lines which lie between the two H's, as well as those which are found in the neighbourhood of G, F, *b* and E.

§ 4. That the theory of the reciprocity of radiation and absorption, to which we have more than once referred in the foregoing pages, is not a mere conjecture, but a logical consequence of the theory of thermometric heat published by me in 1848*, will, I trust, be placed beyond doubt by the following short view of the theory of thermometric heat, as expounded in the paper referred to.

The possibility of a mathematical treatment of thermometric heat always presupposes a greater or less number of hypotheses

* Försök till en matematisk teori för thermometriska värmets (Attempt at a mathematical theory of thermometric heat). Upsala, 1848.

respecting the nature of heat; and at the same time the success of such treatment must depend essentially on the truth of the hypotheses assumed, whence it is evident that, in assuming the grounds for such a theory, it is necessary, as much as possible, to interrogate Nature by experiments. A first clue is offered by the natural connexion which exists between radiant and thermometric heat. Radiant heat must necessarily consist of transverse æthereal vibrations, while, again, those vibrations are dependent on the influence which the molecules of the body itself have upon the particles of the æther. In the mean time experiment shows that the influence of the medium upon the motion of the æther consists in (1) *a constant alteration of the wave's length and velocity*, (2) *a diminution of the amplitudes dependent on the distance*. This diminution is what is called absorption. It cannot, however, take place unless the particles of the body be set in motion, and the heat accordingly become thermometric. In other words, experience shows that radiant heat is absorbed when it passes through any more or less athermanous body, and that the body is thereby heated: if now radiant heat consist in certain undulatory motions of the æther, and these motions are checked by the influence of the particles of the body, then, according to the law of *vis viva*, these particles must themselves acquire a motion. Now, it is true that this does not absolutely prove that these motions are what constitute thermometric heat; but when we reflect, on the one hand, that these are the only changes one can imagine to take place in consequence of the æther's motion, and, on the other hand, that experiments seem to show that the æther itself cannot be the medium of thermometric phenomena, it appears that such a supposition is necessary.

We must, in fact, consider the conducting-power as a function of the conducting medium's elasticity, whether that medium be the body itself or the æther, though in the latter case the directions of greatest or least conductivity must coincide with the axes of optical elasticity. This is, however, not the case with monoclinic crystals, in which the different systems of axes of elasticity have another position. In gypsum and felspar, it is true that the direction of the greatest conducting-power and that of the greatest elasticity as regards acoustic properties coincide, whereas the bisecting lines of the optic axes make, in the case of gypsum, an angle of 24° , in that of felspar 50° , with the said direction. Add to this that thermometric heat always has an influence on the relative position of the particles, by changing the volume and form of aggregation of the mass, and that cause and effect always stand in a mutual relation to each other, whereas radiant heat does not stand in any known con-

nexion with the molecular phenomena of bodies, and it would seem that the hypothesis, that thermometric heat belongs exclusively to the body's own molecules, is unavoidable.

§ 5. The question then becomes, *What is the nature of that oscillatory motion that constitutes thermometric heat?* By a discussion of the differential equations that obtain for wave-motion in general, we easily arrive at the conclusion, that the molecular elasticity of a medium cannot be sufficiently great to allow that medium to assume an undulatory motion in which the periods of oscillation are so short as those of light and heat. The lengths of the waves would be so small as to be equal to, or at least comparable with, the distances of the molecules, and in that case no undulation is possible. In the case of one medium in which we can observe the periods of oscillation, namely, the case of the vibrations of sound, these periods are millions of times greater than those of light and heat.

If we assume, then, that thermometric heat consists in a motion of the pendulum kind in the smallest constituent parts of the body, and deduce the differential equations for these motions, we obtain equations of the same kind as those which occur in the theory of the secular perturbations of the planets, and, for determining the time of an oscillation, we arrive at an equation the degree of which is expressed by three times the number of molecules within the sphere of attraction, and the roots of which are real. The form of these differential equations, which obtain for molecular oscillatory motion, must, however, be independent of the external force whereby the particles are set in motion, whence we may infer *that the nature of the heat must be independent of its source, and dependent exclusively on the body's own molecular forces.* It must be independent even of the body's form and magnitude, provided the latter be greater than the extent of the molecule's sphere of attraction.

The number of vibratory motions possible is thus three times the number of molecules contained within the sphere of attraction. It does not necessarily follow from this that all these different kinds of oscillatory motions always simultaneously exist in the body, any more than that a sounding body should simultaneously assume the whole series of tones that can possibly be produced from it.

When, however, the number of molecules within the sphere of attraction is sufficiently great, the body ought, at a glowing heat, to display a continuous spectrum, as is really in general the case with solid bodies. That gases in general exhibit a discontinuous spectrum is, according to our view of the nature of heat, owing to the circumstance that there are but few molecules within the sphere of attraction.

§ 6. When the bodies are such that a change from one form of aggregation to another occurs without any sensible change in the velocity of the molecular vibrations or any internal operation taking place, the latent heat may be explained by a simple rearrangement and varied grouping of the molecules. If we call m' and m the mass of a molecule in the solid and fluid state of a body whose mass is M , so that

$$\Sigma m' = \Sigma m = M,$$

and if we furthermore put τ' and τ , ρ' and ρ for the molecule's time of oscillation and radius vector, we shall have

$$\Sigma \left\{ \frac{m'}{\tau'} \cdot \int_0^{\tau'} \frac{\delta^2 \rho'}{dt^2} \cdot dt - \frac{m}{\tau} \cdot \int_0^{\tau} \frac{\delta^2 \rho}{dt^2} \cdot dt \right\} = M \cdot L,$$

or

$$\Sigma (m' \cdot A'^2 \cdot s'^2 - m \cdot A^2 \cdot s^2) = M \cdot L,$$

where L expresses the latent heat, and A' , A the amplitudes of the molecule's undulations.

If a change in the form of aggregation take place without a change in the velocities of the molecules' vibrations, then

$$A'^2 \cdot s'^2 = A^2 \cdot s^2,$$

and accordingly

$$\Sigma m' \cdot A'^2 \cdot s'^2 \cdot \left(1 - \frac{m}{m'} \right) = M \cdot L.$$

If we call the body's specific heat in the solid and fluid forms respectively c' and c , then experience shows us that in general

$$m \cdot c = m' \cdot c';$$

and as the zero of the Centigrade scale is arbitrary, we may assume

$$A'^2 \cdot s'^2 = b + t,$$

whence we get

$$(b + t^0) \cdot (c - c') = L, \quad dL/cr = L,$$

if the specific heat of water be taken as the unit.

This formula, which was already known in the time of Crawford and Gadolin, and occurs in their researches on specific heat, has in later times been given by Persoon, who has also verified it for a variety of different bodies.

§ 7. In a memoir "On the Meaning of the Plane of Polarization*," I adduced some experiments to show that, for crystals with different axes of elasticity, the velocity of the molecular vibrations is different in different directions. In experiments with rock-crystal, tourmaline, and felspar (the two last opaque), the heat

* Poggendorff's *Annalen*, vol. xc. p. 582.

which, on the crystals being heated, radiated from a surface cut parallel to the axis of the crystal *, showed no signs of polarization, whereas the heat from a disk of gypsum, parallel and easily permeable, when heated to a temperature at which it lost its water of crystallization, was evidently polarized.

The intensity of the heat that radiates from a surface must depend, 1st, on the velocity of the molecular vibrations ; 2ndly, on the depth under the radiating surface to which the particles of the body can be considered as participating in the radiation : and as that depth must, at least in the case of tourmaline, be different for the vibrations parallel to and perpendicular to the axis of the crystal, it necessarily follows that the amplitude of a vibration must be greatest in that direction in which the depth of the radiating particles is least.

This result has, with respect to tourmaline, been confirmed by Mr. B. Stewart's interesting experiments "On the Nature of the Light emitted by heated Tourmaline" †.

§ 8. In the *absorption* of light and radiant heat, *i. e.* in the transition from an undulatory motion of the particles of the æther to a pendulous motion of those of the medium, the amplitudes of the oscillations must depend (A) on the molecule itself, *its mass and volume*, and (B) on the medium's elasticity, or the *oscillatory movements which are possible in that medium*.

Again, in the transition from the molecular motion of the medium to that of the surrounding æther, *i. e. in radiation*, the connexion stated under the rubric (A), as existing between the amplitude and the mass, must continue to hold ; so that one is thus far justified in saying that *for a given medium the absorbing- and radiating-powers are equal*, a consequence which in fact seems to follow from Provostaye and Desprez's observations. Since, however, the molecules of a body can assume only such oscillatory motions as the medium admits of, and we are therefore obliged to ascribe to the medium different absorbing-powers for different colours, the converse of this theorem cannot be inferred ; for it must be admitted that æther is with equal facility susceptible of all possible undulatory motions. The circumstance that a body most readily radiates just the kinds of light and heat that it absorbs, is not therefore to be ascribed *to any specific difference of radiating-power*, but simply to the fact that the æther cannot assume any other undulatory motion than that which already exists in the radiating medium.

In the preceding pages we have assumed that light and radiant heat on absorption become thermometric : this is not, however,

* Namely, for rock-crystal and tourmaline ; for felspar the radiating surface was at right angles to the symmetrical axis.

† Phil. Mag. May 1861.

the only form in which absorption shows itself; it can, as is well known, appear as *chemical agency, fluorescence*.

The chemical action must, however, be under the same rule as the thermometric heat, viz. that a ray will show a more powerful chemical agency in proportion to the ease with which the smallest constituent parts of which a molecule may be considered as composed assume certain oscillatory motions.

Fluorescence and phosphorescence, on the other hand, cannot be explained on the grounds set forth in the foregoing articles. In the first place, it must be considered as a law of nature that, as long as light and radiant heat preserve their form of undulatory motion, the period of oscillation remains unaltered. This, as appears from the experiments of Melloni and Knoblauch, is also the case with *diffused heat*, which moreover ceases instantaneously with the radiation that produces it. Fluorescence, on the contrary, is similar to thermometric heat, in the circumstance that it is independent of the source of heat, and moreover, according to the researches of Becquerel, does not cease instantaneously on the cessation of the radiation. But, in the next place, since the composition of thermometric heat depends only on the nature of the body itself, and since a certain high temperature, and therefore determinate amplitudes in the vibrations of the body's minutest parts, are always necessary in order that these latter may appear in a state of incandescence, it follows that fluorescent light cannot belong to the body's own molecules. Add to this, that fluorescence is ordinarily of very short duration and gives a discontinuous spectrum, whereas the spectrum from an incandescent solid body is continuous.

The only method, then, that I see of explaining fluorescence is to consider it as generally consisting in pendulous motions of the æther, analogous to those of thermometric heat, or else in permanent undulations. This supposition offers a complete analogy between the phenomena of sound and light; for as the air can at once propagate sound and be itself the resonant body, so can æther at once propagate the light-wave and be itself the cause of it.

II. *On Nerve Force.—Relation of Nerve Force to Electric Force.* *Origin of Nerve Force.* By H. F. BAXTER, Esq.*

HAVING partly considered the present question on a former occasion, I will briefly recapitulate the conclusions that were then arrived at, referring my readers to the published papers† for the arguments upon which they were established.

* Communicated by the Author.

† Organic Polarity. Churchill, London, 1860. Edinburgh New Philosophical Journal, July 1860.

First. The nervous tissue, like the muscular tissue, possesses an electric state.

Secondly. This electric state is developed during, and maintained by, nutrition.

Thirdly. The electric force of the nervous tissue is not *converted* into *current force* during nerve action.

Fourthly. Whether the electric force of the tissue is *converted* into *nerve force* during nerve action was left an open question. It is this question I propose considering on the present occasion.

§ I. *On the conversion of the electric force of the nervous tissue into nerve force during nerve action.*

Assuming my readers to be acquainted with the conditions under which the so-called nerve current is obtained in the living or recently-killed animal, viz. by placing the electrodes of a galvanometer, one in contact with the transverse section, the other in contact with the longitudinal surface of a nerve, the point for consideration is this:—Can we obtain any evidence of a *sudden* decrease in the nerve current *during* nerve action indicative of a *loss* in the electric state of the nerve, and which loss may give rise to the development of nerve force,—in other words, a *conversion* of the electric force into nerve force in accordance with the principle of *Conservation of Force**?

To avoid unnecessary repetition I will sum up the general results of my experiments, as my object will be, not to study the development of the nerve current, but merely the effect upon it during the excitement of the nerve.

The animal (guinea-pigs, rabbits, and frogs) having been pithed, to avoid too great motion of the limb, the leg was fastened down; the sciatic nerve was then exposed throughout its whole course, and a piece of thin glass placed beneath it. One of the electrodes of the galvanometer being pointed and coated with shell-lac, excepting the extreme end, was inserted into the nerve; the flat end of the other electrode was placed on the surface of the nerve, sometimes on the distal side, at other times on the spinal side of the other. The distance between the electrodes varied. As soon as the circuit was completed and the nerve current obtained and noted, the spinal extremity of the nerve was excited by a small galvanic circle, or by a single cell of Grove's battery, so as to produce violent contractions in the muscles of the lower limb. The effect upon the galvanometer needle was that of a slight oscillation; there was no *sudden* or

* Faraday "On the Conservation of Force," Philosophical Magazine, April 1857. Professor Helmholtz "On the Application of the Law of Conservation of Force to Organic Nature," Medical Times and Gazette, April 20, 1861.

marked decrease in the nerve current ; and the effect, the oscillation of the needle, was evidently due to the motion produced in the nerve by the muscular contractions. Instead of exciting the spinal extremity of the nerve, the sole of the foot was irritated by pieces of glass so as to cause reflex actions ; the effects were the same.

In other experiments the *distal* extremity of the sciatic nerve was divided, and the electrode, instead of being inserted into the nerve, was placed in contact with the divided surface, the other electrode being placed upon the external or longitudinal surface ; there was not the same oscillation of the needle as before, neither was there any marked or *sudden* decrease ; the needle gradually receded.

The nerve, and sometimes the spinal cord, was irritated by other means (chemical* and mechanical) so as to produce contractions ; but the results were the same. It need scarcely be stated that, when employing these means to excite muscular contractions, great care is requisite that the chemical reagents, or the electrodes of the battery, should not come into contact with the electrodes of the galvanometer, and that the mechanical agent should not consist of steel or iron, so as to act upon the needle of the galvanometer. In short, in all these and similar experiments where the galvanometer is employed, no steel should be about the person, such as covered metallic buttons in the sleeves of the coat, or penknives in the pockets ; nor should the shade of the galvanometer be exposed to the heating effect of the sun's rays.

Before quitting this part of my subject, I may just allude to the question respecting an *increase* in the nerve current during nerve action ; but having considered it on a former occasion, I need do no more than refer to my published paper† containing an account of the experiments in which I failed to obtain any decided evidence of an *increase* in the nerve current during nerve action.

From the results obtained in the foregoing experiments, I can come to no other conclusions than the following :—

1st. That no *sudden* or marked *decrease* in the electric condition of the nerve occurs *during* nerve action.

2ndly. That the *decrease* which is observed arises from a disorganization of the nerve.

3rdly. That there is no evidence of a *conversion* of the electric force of the tissue into nerve force *during* nerve action.

* In a Report on Muscular Contraction, by A. B. Duffin, M.D., published in Professor Beale's 'Archives of Medicine' for January 1862, will be found some valuable results obtained by Kühne, respecting the action of the different chemical reagents upon nerves and upon muscles.

† *Loc. cit.*

4thly. That the electric state of the nerve may be considered as one of the properties of the nerve, but not its *essential* property. And

5thly. That nerve force is a higher form of force than electric force—a force *sui generis*.

Although the foregoing conclusions lead to the inference that nerve force is not so dependent upon the electric state of the tissue as to be considered as cause and effect, we must not overlook the fact that an intimate *connexion* exists between these two forces, as manifested in the development of the force in the fish. Here, however, the connexion is not so immediate as to indicate a *conversion* of nerve force into electric force, as there is reason to believe, from some experiments of Matteucci*, that the electric force pre-exists in the organ. And with regard to the muscular tissue, the force which is evolved during muscular contraction will be found, I believe, to arise from the force preexisting in the muscular tissue, rather than from a conversion of nerve force into electric force. It is of the utmost importance, in discussing questions respecting the “connexion of forces” or “the correlation of forces,” to ascertain how far the manifestation of the forces are independent of each other, before we can apply the term *conversion* to their explanation. A *connexion* may be frequently shown to exist between the forces where we may feel some difficulty in applying the term *conversion*, and consequently the employment of the former term may in many instances be the most appropriate.

As the principle of “conservation of force” is now being applied to organic phenomena, it behoves us to endeavour to obtain clear views regarding the *origin* of the various forces that are manifested in the living organism; and although it may be safely asserted that, whatever forces are manifested in the living organism, they are greatly if not entirely dependent upon nutrition for their origin and maintenance, it nevertheless becomes necessary to examine into the mode of their production. And here I would observe that in speaking of forces the term may be employed in an abstract or concrete sense; and although it may frequently be more convenient to employ it abstractedly, I do not consider forces as entities, but rather as properties connected with the various tissues. Now with regard to the origin of nerve force, the following question arises, Is it *generated* in any particular part of the nervous system? or is it associated with the nervous matter wherever formed?

§ II. On the Origin of Nerve Force.

The late Dr. Todd was the first, I believe, who distinctly

* Proceedings of the Royal Society, Nov. 1860.

stated that nerve force was a *polar force**. He considered the nervous substance to consist of two forms—the vesicular or grey nervous matter, and the fibrous or white nervous matter. But with regard to the generation of nerve force, he compared it to the actions which take place in a galvanic battery. He says†, “At the point of junction or intermingling of these two forms of nervous matter (the white and the grey or vesicular) blood is very freely supplied, and at these points there is generated constantly and unceasingly a force which in its nature resembles very closely the galvanic force or current electricity as developed in the galvanic battery or magneto-electric machine But there are ample grounds for the opinion that, although nearly allied to electricity, it differs from, and is probably a higher relation of force than, that power.”

Dr. Carpenter‡ says, “There can be no reasonable doubt that the production of nerve force in the central organs is dependent upon the development of the peculiar cells constituting the ganglionic or vesicular substance; that either cells or cell-nuclei are the agents in the origination of nerve force at the peripheral extremities of the nerve fibres.”

According to Müller§, “the nervous principle is generated and regenerated in the central organs.”

Professor Huxley||, in a lecture delivered at the Royal Institution, alludes to the rhythmic contraction of the heart, and states, “we know that the ganglia are centres whence that excitement (nervous) originates.” And Mr. Paget¶ speaks of the ganglia of the heart as being the rhythmic centres, and at the same time refers the effects (rhythmic contractions) to rhythmic nutrition.

Without quoting other authorities, I believe the general opinion amongst anatomists and physiologists to be the following:—that the vesicular or ganglionic portion of the nervous system originates nervous force, and that it is transmitted from one part of the nervous system to another by means of the fibrous or white portion. I will not say that this is the opinion of *all* physiologists, or that I am acquainted with the views that are generally entertained upon the subject; but as far as I have been able to understand the language that has been employed, it appears to me to be implied that nerve force is generated in

* Cyclopædia of Anatomy and Physiology, art. Nervous System.

† Medical Gazette, April 27, 1849.

‡ “On the Mutual Relations of the Vital and Physical Forces,” Phil. Trans. 1850.

§ Müller’s Physiology, trans. by Baly, 2nd edit. p. 847.

|| On the Present State of Knowledge as to the Structure and Functions of Nerve, May 15, 1857.

¶ Proceedings of the Royal Society, May 28, 1857.

one part and transmitted to another. Mr. Huxley, however, after describing the connexion existing between the ganglionic corpuscle and the nerve-fibres, very pertinently asks, "What are the properties of ganglionic corpuscles?" This appears to me to be the very question requiring solution. Do they *originate* nerve force which is transmitted by the nerves to other parts? or do they merely serve as the means of intercommunication with the various parts of the nervous system, the whole extent of the nervous system originating nerve force?

The evidence upon which it is generally assumed that nerve force arises in one part and is transmitted to another, rests upon the non-transmission of *impressions* when the continuity of the nervous trunk is interrupted either, as in our experiments, by section or removal, or by disease. Here, however, we assume the non-transmission of *impressions* to be identical with the non-transmission of *force*, and therefore it leaves the question as to the parts originating nerve force undecided. Brown-Séquard has been obliged to draw "a distinction between the property of conduction or transmission and the property of being sensitive or impressionable." He says, "The nerve fibres of the cerebral lobes are *conductors*, but they are not *excitable*, not *impressionable*; and so is the grey matter of the spinal cord: when it exists alone, establishing the communication between two parts of the spinal cord, after a transverse section of the whole of the white matter, it *conducts*, it *transmits* to the brain the sensitive impressions made on *impressionable* organs *behind* the section, but when irritated it does not transmit anything, because it is not *impressionable**." Here, then, we are obliged to suppose the same property, the same mode of conduction to be associated with two portions of the nervous system presenting such different anatomical characters.

Assuming it possible that nerve force might be generated in the ganglionic portion as suggested by Dr. Todd, it appeared probable that a difference in the nutritive actions in it, when compared with those of the white fibrous matter, might be observed—that a greater activity would be manifested, and hence produce a more powerful nerve current. To ascertain this point the following experiments were performed.

§ III. *On the electric condition of the ganglionic or vesicular portion of the nervous substance.*

In order to lay open the skull easily and readily, young animals (guinea-pigs and rabbits) were used, and after being pithed the brain was exposed, a section made, and one electrode placed in contact with the divided surface, whilst the other was

* Lecture II.

placed on the surface of the convolution; the effect upon the needle amounted to 3° . There was no difference in this respect whether the experiment was made on the convolutions of the cerebrum or those of the cerebellum. The greatest care, however, is required not to allow one of the electrodes to come into contact with the blood from a wounded vessel, as the effect upon the needle was then very decided, amounting to 10° or 15° or more. Here, then, in these experiments we obtained the same effects (the nerve current) as with the two surfaces of a nerve, and, what is more to the purpose, the amount is not greater than with a nerve.

It was impossible to perform similar experiments with the grey matter of the spinal cord, inasmuch as the two surfaces are divided sections. When, however, one electrode was placed on the longitudinal surface of the cord, and the other in contact with the divided surface, no difference was observed when the latter electrode was placed in contact with the grey matter or with the white matter of the cord; the longitudinal surface was positive to the other as with a nerve.

Similar experiments were made on the cervical ganglia of rabbits and guinea-pigs, by placing the electrodes one on the surface, and the other in contact with the divided surface; the effect upon the needle amounted to 2° or 3° .

In all these experiments I could obtain no evidence to lead to the supposition that the electric condition of the vesicular or ganglionic portion of the nervous system differed from that of the white or fibrous, or that the *intensity* of the nerve current was such as to lead to the conclusion that the nutritive actions were more active in the ganglionic than in the white portion.

It may be said that the foregoing experiments do not show or prove that the vesicular or ganglionic portion is not the source of nervous power, but only that nerve force is not generated like galvanic force. I grant these objections, and am willing to allow them their full force; but they prove this fact, that there is not that difference between the two portions of nervous matter to lead us to suppose that they are endowed with distinct properties.

§ IV. *Concluding Remarks.*

Let us now consider the grounds upon which it is believed that nerve force is generated in the central parts (the ganglionic) of the nervous system; the evidence will resolve itself into this: if from any cause the influence of the will be not transmitted to the limb so as to cause contractions, it is tacitly assumed that this arises from the non-transmission of nerve force to the part, and consequently that there must be some part in which this force is *generated*, prior to its being transmitted. I will not say

that these are the views that are generally entertained ; but the language usually employed implies them. The experiments of Helmholtz, in which he has calculated the time that elapses in the conveyance of an impression from one part of the nervous system to another, and the analogy that has been attempted to be drawn between a current of electricity traversing a wire in a galvanic circle, and the force which is supposed to be conducted by a nerve, have afforded some grounds for the supposition ; but how could we explain reflex actions unless we supposed that the force was generated at the peripheral extremities of the nerves ? Again, if nerve force be generated in any particular part of the nervous system, how is it that we are enabled to excite contractions in the muscles of a limb by stimulating the divided extremity of the nerve ? We cannot suppose for a moment that the mere irritation is *converted* into nerve force ; this would be carrying the *conversion*-doctrine to rather an absurd extent.

The supposition that nerve force is *generated* in the vesicular or ganglionic portion only of the nervous system and then transmitted to other parts, I believe to be erroneous. That the peripheral extremities of the nerves, distal and central, are more adapted to receive impressions than the white or fibrous substance, I admit. Impressions are made at the peripheral extremities of the nerves, and then transmitted to other parts by means of the white fibrous substance, or even by the grey or ganglionic portion ; but the force, nerve force, is generated or associated with the nervous tissue under whatever form it assumes.

Although I have failed in establishing to my own satisfaction the development of nerve force from electric force, still the application of the principle of *Conservation of Force* must not be lost sight of by physiologists. We have a right to ask how a force is generated, and how exerted or expended ; we have no right to assume that nerve force is *increased* by action ; *action* implies exhaustion. I have elsewhere stated that nerve force may be considered in a state of tension, and that it bears a stronger resemblance to magnetism than to current force. Its loss by action, nervous exhaustion, is restored by *nutrition* ; and increased nutrition may give rise to excess of nerve force ; but during health the various forces in the organized body are so admirably adjusted, that in their action and reaction upon each other no disturbance ensues ; in disease, however, the disturbance may manifest itself in various forms.

There is one other observation to make before concluding. We shall err greatly if we keep limiting our inquiries to the mere study of the electric properties of the various tissues, just as we should err if we limited them to the investigation of the heat

that is developed in the animal body, or the carbonic acid in the lungs, or the urea in the kidneys. The study of one is of as much importance as that of the other. All these actions are mutually dependent and answer one common purpose—the well-being of the individual.

III. On the Transcendent $gd . u = \frac{1}{i} \log \tan (\frac{1}{4}\pi + \frac{1}{2}ui)$.

By A. CAYLEY, Esq.*

THE elliptic functions which correspond to the modulus $k=1$ reduce themselves, as is well known, to circular functions. The case in question plays implicitly an important part in the general theory, and it has been particularly studied by Gudermann, and by Dr. Booth in connexion with his theory of parabolic logarithms. But in the absence of a notation corresponding to that used for elliptic functions in general, the theory has not, it appears to me, been exhibited in its proper form. The defect is very easily supplied: using for $am . u$, to the modulus 1, the notation $gd . u$ (Gudermannian of u), then if

$$u = \int_0^\phi \frac{d\phi}{\cos \phi} = \log \tan (\tfrac{1}{4}\pi + \tfrac{1}{2}\phi),$$

we have

$$\phi = gd . u.$$

And hence, observing that the equation between u and ϕ is

$$u = \log \frac{1 + \frac{1}{2} \tan \phi}{1 - \frac{1}{2} \tan \phi},$$

or, what is the same thing,

$$\tan \frac{1}{2} \phi = \frac{e^u - 1}{e^u + 1},$$

and that we have

$$\begin{aligned} \log \tan \left(\frac{\pi}{4} + \frac{1}{2} ui \right) &= \log \frac{1 + \tan \frac{1}{2} ui}{1 - \tan \frac{1}{2} ui} \\ &= \log \frac{\cos \frac{1}{2} ui + \sin \frac{1}{2} ui}{\cos \frac{1}{2} ui - \sin \frac{1}{2} ui} \\ &= \log \frac{e^{\frac{1}{2}u} + e^{-\frac{1}{2}u} + i(e^{\frac{1}{2}u} - e^{-\frac{1}{2}u})}{e^{\frac{1}{2}u} + e^{-\frac{1}{2}u} - i(e^{\frac{1}{2}u} - e^{-\frac{1}{2}u})} \\ &= \log \frac{e^u + 1 + i(e^u - 1)}{e^u + 1 - i(e^u - 1)} \\ &= \log \frac{1 + i \tan \frac{1}{2} \phi}{1 - i \tan \frac{1}{2} \phi} = \log e^{i\phi} = i\phi, \end{aligned}$$

* Communicated by the Author.

or if

$$u = \log \tan \left(\frac{1}{4}\pi + \frac{1}{2}\phi \right),$$

then

$$\phi = \frac{1}{i} \log \tan \left(\frac{1}{4}\pi + \frac{1}{2}ui \right).$$

And substituting for ϕ its value, we obtain

$$gd . u = \frac{1}{i} \log \tan \left(\frac{1}{4}\pi + \frac{1}{2}ui \right),$$

which is the definition of the transcendent $gd . u$. It is to be noticed that $gd . u$, although exhibited in an imaginary form, is a real function of u ; and, moreover, that it is an odd function, viz. we have

$$gd(-u) = -gd(u),$$

and therefore also

$$gd(0) = 0.$$

The original equation,

$$u = \log \tan \left(\frac{1}{4}\pi + \frac{1}{2}\phi \right),$$

written under the form

$$u = i \frac{1}{i} \log \tan \left(\frac{1}{4}\pi + \frac{1}{2}i \frac{\phi}{i} \right),$$

shows that we have

$$u = i gd \left(\frac{\phi}{i} \right) = i gd(-i\phi);$$

or substituting for ϕ its value $gd . u$, we have

$$u = i gd(-i gd . u),$$

which may also be written

$$iu = gd . i gd . u.$$

So that $gd . u$ is a quasi-periodic function of the second order—a property which has not, at least obviously, any analogue in the general theory. We have

$$\begin{aligned} \cos gd u &= \frac{1}{2} (e^{i gd . u} + e^{-i gd u}) \\ &= \frac{1}{2} \left(\frac{1 + \tan \frac{1}{2} ui}{1 - \tan \frac{1}{2} ui} + \frac{1 - \tan \frac{1}{2} ui}{1 + \tan \frac{1}{2} ui} \right) \\ &= \frac{1 + \tan^2 \frac{1}{2} ui}{1 - \tan^2 \frac{1}{2} ui} = \frac{1}{\cos ui}. \end{aligned}$$

And in like manner

$$\begin{aligned}\sin gd u &= \frac{1}{2i} (e^{i gd u} - e^{-i gd u}) \\ &= \frac{1}{2i} \left(\frac{1 + \tan \frac{1}{2} ui}{1 - \tan \frac{1}{2} ui} - \frac{1 - \tan \frac{1}{2} ui}{1 + \tan \frac{1}{2} ui} \right) \\ &= \frac{2 \tan \frac{1}{2} ui}{i(1 - \tan^2 \frac{1}{2} ui)} = \frac{\sin ui}{i \cos ui},\end{aligned}$$

or, as these equations may also be written,

$$\begin{aligned}\sec gd u &= \cos ui = \frac{e^u + e^{-u}}{2}, \\ \tan gd u &= \frac{1}{i} \sin ui = \frac{e^u - e^{-u}}{2}.\end{aligned}$$

And from these equations we have

$$\sec gd(u+v) = \sec gd u \cdot \sec gd v + \tan gd u \cdot \tan gd v,$$

$$\tan gd(u+v) = \tan gd u \cdot \sec gd v + \tan gd v \cdot \sec gd u;$$

or, what is the same thing,

$$\begin{aligned}\sin gd(u+v) &= \frac{\sin gd u + \sin gd v}{1 + \sin gd u \cdot \sin gd v}, \\ \cos gd(u+v) &= \frac{\cos gd u \cdot \cos gd v}{1 + \sin gd u \cdot \sin gd v};\end{aligned}$$

which form is at once obtainable from the formulæ

$$\sin am(u+v) = \frac{\sin am u \cos am v \Delta am v + \sin am v \cos am u \Delta am u}{1 - k^2 \sin^2 am u \sin^2 am v},$$

$$\cos am(u+v) = \frac{\cos am u \cos am v - \sin am u \sin am v \Delta am u \Delta am v}{1 - k^2 \sin^2 am u \sin^2 am v}$$

$$\Delta am(u+v) = \frac{\Delta am u \Delta am v - k^2 \sin am u \sin am v \cos am u \cos am v}{1 - k^2 \sin^2 am u \sin^2 am v},$$

observing that for $k=1$ we have $\Delta am = \cos am$, and that the numerators and denominator each of them divide by

$$1 - \sin am u \sin am v.$$

IV. *Chemical Notices from Foreign Journals.*By E. ATKINSON, *Ph.D., F.C.S.*

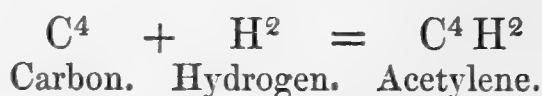
[Continued from vol. xxiii. p. 478.]

OF all the carburets of hydrogen, acetylene, C^4H^2 *, is the most stable. It can be formed from olefiant gas or marsh-gas by the action of heat, or by that of the induction spark; and it is also formed under the influence of the former agent, from benzole and naphthaline, which have usually been regarded as the most stable carburets. Berthelot† has succeeded in preparing this gas artificially by the direct union of its elements.

Hydrogen may readily be obtained pure and dry; but this is not the case with carbon, which usually contains a variable proportion of hydrogen. This, Berthelot found, was best removed by the action of chlorine at a high temperature, which has further the advantage of removing sulphur, aluminium, and silicon in the form of volatile chlorides.

Purified carbon is not acted upon by hydrogen under the influence of the highest heat alone, even when carried to the point of melting the porcelain tube in which the carbon was placed. Nor were experiments made with the induction spark more successful.

But when the electric arc is formed between two charcoal electrodes in an atmosphere of hydrogen, combination takes place as soon as the spark commences to pass. Acetylene is formed, and is the sole product of the reaction, and its production continues as long as the electric arc is maintained; it can be reproduced indefinitely by the same charcoals so long as the transport of matter between the two poles has not entirely dis-aggregated them.



The acetylene formed around the poles is carried away by the current of hydrogen, and is condensed in an ammoniacal solution of protochloride of copper, producing a red precipitate of cuprous acetylene.

It is easy to obtain large quantities of acetylene in this way; under the conditions of the experiment, Berthelot was able to prepare about 10 cubic centims. of acetylene in a minute.

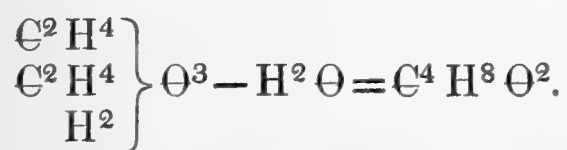
The compound of acetylene with copper was treated with pure hydrochloric acid, by which acetylene was liberated in the free state; both its analysis and the comparison of its properties identified it.

* *Phil. Mag.* vol. xx. p. 196.† *Comptes Rendus*, March 24, 1862.

Acetylene is not an isolated substance, but presents a starting-point for the formation of other bodies. Berthelot has shown that by simple addition of hydrogen it can be changed into olefiant gas; by means of olefiant gas, alcohol can be formed, and thus a commencement made with the chain of compounds which constitute organic chemistry. To all syntheses and progressive formations, that of acetylene furnishes the first step.

In continuation of his researches on oxide of ethylene, Wurtz* tried the action of bromine on this substance. When oxide of ethylene was mixed with a suitable quantity of bromine, and the mixture kept cool, it was transformed into a mass of red crystals impregnated by a thick red mother-liquor. This was poured off and the crystals pressed between porous earthen plates. These crystals melt at 65° to a deep-red liquid, which boils at 95° , emitting an orange vapour, which on condensing reproduces the liquid and the red crystals. The crystals are insoluble in water, but dissolve in alcohol and in ether. The composition of the

crystals is expressed by the formula $\left. \begin{matrix} \text{C}^2 \text{H}^4 \Theta \\ \text{C}^2 \text{H}^4 \Theta \end{matrix} \right\} \text{Br}^2$,—an instance of the simple addition of bromine to an oxidized body, the molecule oxide of ethylene being doubled in combining. The bromine is but loosely combined, and may readily be removed either by sulphuretted hydrogen or metals, of which the most convenient is mercury. The body formed by this reaction is a colourless liquid, with a feeble but pleasant odour. At a low temperature, it solidifies to a crystalline mass, which melts at $+9^{\circ}$. It boils at 102° . Analysis and a determination of the vapour-density showed that its composition is $\text{C}^4 \text{H}^8 \Theta^2 = \left\{ \begin{matrix} \text{C}^2 \text{H}^4 \Theta \\ \text{C}^2 \text{H}^4 \Theta \end{matrix} \right\}$; it is oxide of ethylene doubled, or *dioxyethylene*. It is an isomer of the compound of aldehyde and oxide of ethylene discovered by Wurtz, and may probably be regarded as the ether of diethylenic alcohol,



It is completely soluble in alcohol, ether, and water. It combines with difficulty with anhydrous acetic acid.

Wurtz placed an aqueous solution of oxide of ethylene in a cooled vessel containing sodium-amalgam. On the following morning the aqueous alkaline liquid was distilled, and the distillate rectified over carbonate of potash. It had the properties and

* *Comptes Rendus*, February 10, 1862.

composition of ordinary alcohol—one of the infrequent examples of the formation of a body by the direct addition of hydrogen,



Cannizzaro and Rossi* have obtained some of the radicals of the aromatic-alcohol series by methods analogous to those by which the radicals of the ethylic-alcohol series are obtained, namely, by the action of metals on the chlorides of the alcohol radicals.

When excess of sodium is added to chloride of benzyle and the mixture heated to 100° , the metal becomes of a violet-blue colour, while the liquid is yellow and of a very thick consistence. The mixture is shaken with ether, which dissolves all organic substances, and on evaporating the solution a yellow oily substance remains, which after some time crystallizes in laminæ and needles. This is the radical of benzoic alcohol in an impure condition, and is purified by being pressed between bibulous paper and recrystallized two or three times from strong alcohol. *Benzyle*, $\text{C}^{14} \text{H}^{14}$, is a white crystalline substance which melts at 52° and boils at 284° . It is insoluble in pure water, but tolerably so in boiling alcohol, from which it crystallizes in needles.

The radical of cuminic alcohol, *cuminethyle* or *cuminyle*, is obtained in quite the same way; the action of sodium on chloride of cuminyle is attended with the evolution of heat. The radical is a white substance, which both on cooling from a hot saturated alcoholic solution, and on the spontaneous evaporation of its ethereal solution, is obtained in broad thin laminæ, with a mother-of-pearl lustre. It is insoluble in water, but tolerably soluble in cold, and still more readily in hot alcohol, in ether, and in bisulphide of carbon. Its composition is expressed by the formula $\text{C}^{20} \text{H}^{26}$.

The authors also appear to have obtained the radical of anisic alcohol, but not in quantities sufficient for accurately establishing its composition and properties.

Wurtz has described† a new method of forming amylene. Zinc-ethyle and iodide of allyle were enclosed together in sealed tubes and heated in the water-bath. A brisk action took place, and the sides of the tube became covered with a crystalline mass of iodide of zinc. On opening the tube a considerable quantity of gas was disengaged, and the whole of the liquid distilled over in the water-bath. It was then heated in a sealed tube with potassium, to decompose the iodide of allyle, and the product of this

* Liebig's *Annalen*, February 1862. *Comptes Rendus*, vol. liii. p. 541.

† *Comptes Rendus*, February 17, 1862.

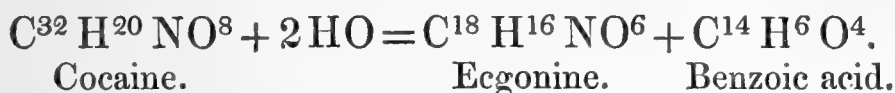
treatment submitted to fractional distillation, by which it was resolved into three hydrocarbons, hydride of amyle, amylene, and allyle, the latter being the most abundant. It boils at 59° , and is easily separated from the two others which boil at 30° and 35° respectively. These are separated by treatment with bromine, with which amylene forms a compound, while hydride of amyle is quite unattacked.

The formation of amylene is effected by a simple addition of the radical ethyle to the radical allyle,



and is thus a sort of mixed radical, *ethyle-allyle*, analogous to those described by Wurtz some years ago.

The investigation of cocaine, the new base in the coca leaf, begun by Niemann under Wöhler's direction*, has been continued by Lössen†. When the base is treated with hydrochloric acid, it undergoes a simple metamorphosis: it separates into benzoic acid, and a new base, for which Wöhler proposes the name *Ecgonine* (ἐκγονος, *sprout*). The analysis of this new base indicated that its formula was $\text{C}^{18}\text{H}^{16}\text{NO}^6$, and hence that the decomposition of cocaine by hydrochloric acid took place in the manner represented by the following equation:—



The decomposition is best effected by heating the solution of cocaine in a sealed tube for some time to a temperature of 100° .

The new base is soluble in water, and is not precipitated by carbonate of soda nor by bichloride of platinum. But if to a mixture of the base with bichloride is added a large quantity of alcohol, the double salt crystallizes in long orange-yellow needles.

The above deportment of cocaine explains why, on extracting the leaves with dilute acid, in many cases no base is obtained. The best method of obtaining cocaine is, according to Lössen, the following:—The leaves are digested with rain-water for some hours between 60° and 80° , the infusion poured off, and the operation repeated several times. The united infusions are heated with acetate of lead, the precipitate filtered off, and the excess of lead removed by the addition of sulphate of soda. The liquid filtered from the precipitated sulphate of lead is made alkaline with carbonate of soda, and exhausted four to six times with ether. The greater part of the ether is distilled off from the ethereal extracts; on spontaneous evaporation cocaine is left in

* Phil. Mag. vol. xx. p. 141.

† Liebig's *Annalen*, March 1862.

an impure condition, and may be purified by the method described by Niemann.

Lössen tried the use of amylic alcohol in extracting the free base, but did not find it advantageous. It led, however, to the discovery of a new base, which seems to exist preformed in the plant. It is a liquid, and Wöhler gives to it the name *Hygrine* (*ὕγρὸς*, *liquid*). It may be distilled with water; its odour is like that of trimethylamine; it is strongly alkaline, but not bitter, and forms white fumes with volatile acids. Its hydrochlorate crystallizes, but is very deliquescent. With bichloride of platinum it gives a flocculent non-crystalline precipitate. Hygrine is not poisonous; a few drops administered to a puppy were without apparent action.

Troost* has made another determination of the atomic weight of lithium, having discovered, by means of spectrum-observations, that the salts of lithium which he had previously used for this determination were not absolutely pure.

He prepared pure carbonate of lithia by precipitating chloride of lithium by carbonate of ammonia; the precipitate, washed and dried, was suspended in water, through which a current of carbonic acid was transmitted. It rapidly dissolved, and was reprecipitated in the crystalline state when the liquid was boiled. A new solution and new precipitation gave a preparation in which spectrum-analysis showed no trace either of sodium, cæsium, or rubidium.

This carbonate was converted into chloride by heating it in a hard glass tube in a current of hydrochloric acid. The chloride of lithium was precipitated by means of nitrate of silver, and in this way the numbers 7.030 and 6.99, in the mean 7.01, were obtained for the equivalent of the substance. To control this result by another method, the carbonate was decomposed, in one case by sulphuric acid, in another case by heating it with an excess of pure pulverized quartz: this latter method gave the numbers 7.00 and 7.02, in the mean 7.01; the former method in two determinations gave the number 7.06 as that of the equivalent of lithia.

These results thus confirm those obtained by Diehl, who found the number 7.026.

Deville† has described a method by which he has obtained Levyne artificially. Two solutions were prepared, one of silicate of potash, and the other of aluminate of soda, in such proportions that the oxygen contained in the silica was to that in the alu-

* *Comptes Rendus*, February 17, 1862.

† *Ibid.* February 17, 1862.

mina as 2 : 1. The substances were placed in a sealed tube, and at once solidified in the cold. Heated to about 170° , the magma was changed into a colourless liquid, which was separated by decantation from small limpid hexagonal tables; under the polariscope these exhibited the coloured rings and black cross characteristic of the rhombohedral system. The analysis showed that this substance was Levyne, $\text{Al}^2\text{O}^3\text{SiO}^3\text{CaO SiO}^3 + 6\text{HO}$.

When the same mixture was heated to a higher temperature, a sand was produced which was nothing but crystallized silica, accompanied by a small quantity of Levyne.

By altering the relative proportions of silica and alumina, variable results were obtained. A mixture of silicate and aluminate of potash gelatinized at the ordinary temperature, but was transformed at 200° into a crystalline sand which had the composition of Phillipsite.

Deville has found that aluminate of baryta may be formed, either by calcining the nitrate or carbonate of baryta with an excess of anhydrous alumina, or by precipitating sulphate of alumina by baryta.

M. Gaudin has observed* that when steam is passed over a mixture of ferruginous alumina, charcoal, and chloride of barium, or, what is more economical, sulphate of baryta, an aluminate of baryta is formed; the sulphuric acid of the sulphate of baryta passes off in the form of sulphide of carbon, sulphur, sulphurous acid, and sulphuretted hydrogen. The aluminate of baryta is soluble, differing in this respect from the aluminates of lime and magnesia. Lime-water added to the solution produces an immediate precipitate of aluminate of lime.

Aluminate of baryta is well fitted for preparing solution of pure alumina: it is merely necessary to add to the solution sufficient sulphuric acid to precipitate the baryta; the alumina precipitates at the same time; but by adding either excess of sulphuric acid, or acetic or nitric acid, the alumina passes into solution, while the sulphate of baryta remains undissolved.

According to Wöhler†, when nitrate of soda is heated in closed vessels with binoxide of manganese, no manganate of soda is formed,—the reason doubtless being that nitrate of soda is decomposed before the temperature is attained at which manganic acid is formed. The decomposition is so complete that this deportment may be used for the preparation of pure caustic soda.

By fusing together tungstate of soda with chloride of calcium or chloride of lead, Manross obtained the tungstates of these

* *Comptes Rendus*, March 24, 1862.

† *Liebig's Annalen*, September 1861.

bases crystallized exactly in the forms in which they occur in Scheelite; and Geuther and Forsberg* have obtained a series of tungstates by the same method. They found that the addition of common salt is necessary in order to obtain large crystals. They have prepared the tungstates of *baryta*, *magnesia*, *zinc*, and *cadmium*, and find that these bodies are isomorphous with the lime and lead salts. They also prepared the tungstates of *iron* and *manganese*, as well as some of their double salts. These latter bodies crystallize in rhombic forms, like the native tungsten. By using different quantities of the chlorides of manganese and iron, the various natural tungstens may be obtained in beautiful large crystals.

The preparation was effected by placing a mixture of the salts in a covered porcelain crucible, which stood in a Hessian crucible half-filled with magnesia. This was heated in a wind-furnace, and allowed to cool slowly; when the fused mass was extracted with water, the crystals were left undissolved.

Of the various double tungstates of iron and manganese prepared, the compound $7(\text{MnO WoO}^3) + \text{FeO WoO}^3$ was obtained in small brown crystals. It probably corresponds to a Wolfram from Schlackenwalde analysed by Rammelsberg. The compound $2(\text{MnO, WoO}^3) + 3(\text{FeO WoO}^3)$ was obtained in crystals 5 to 6 millims. in length. It corresponds to no natural compound. The compound $\text{MnO WoO}^3 + 4(\text{FeO WoO}^3)$ corresponds to a Wolframite from Ehrenfriedensdorf; and another, with the composition $\text{MnO WoO}^3 + 7\text{FeO WoO}^3$, to a Wolfram from Neudorf, analysed by Rammelsberg. A compound was obtained in lamellar crystals which had the formula $\text{MnO WoO}^3 + 3\text{FeO WoO}^3$; it corresponds to the Wolfram of Limoges, and to a specimen from Cumberland analysed by Berzelius. Attempts were made, but without success, to obtain crystallized tungstate of chrome and tungstate of silver.

In an investigation of the higher degrees of oxidation of bismuth, Schrader† has arrived at the following results:—

In the cold, hypochlorite of potash exercises no oxidizing action on oxide of bismuth.

If hydrated oxide of bismuth be boiled with an alkaline hypochlorite, substances richer or poorer in oxygen are obtained, according as potash is present in greater or less excess.

The bodies obtained by treatment with dilute alkalies are soluble in concentrated nitric acid; treated with dilute acid, brown anhydrous bodies are formed which correspond to the formula BiO^4 .

When boiled with concentrated alkali of the specific gravity

* Liebig's *Annalen*, December 1861.

† Ibid. February 1862.

1.385, according to the proportion of the potash to the oxide of bismuth, either red and yellow substances rich in oxygen are obtained, or brown and black bodies poorer in oxygen. Boiled with concentrated nitric acid, they leave a yellow residue of the composition $\text{BiO}^4 + 2\text{aq}$, *difficultly soluble* in this acid.

Whatever the proportion of the potash to the bismuth, provided very concentrated potash was used, red bodies were obtained, and never, as Arppe states, brown anhydrous bismuthic acid.

The final product of decomposition of these red bodies by concentrated nitric acid is also $\text{BiO}^4 + 2\text{Aq}$.

Liebig has described* the following simple method of preparing iodide of lithium and certain other iodides. One part of amorphous phosphorus is placed in a mortar with forty times its weight of warm water; twenty parts of iodine are added, and brought well in contact with the phosphorus by rubbing. The liquid, at first dark brown, afterwards becomes colourless, and more rapidly if heated in the water-bath. The liquid is poured off from the small residue of phosphorus and quite saturated with baryta, at first with carbonate, and afterwards with baryta-water; and then filtered from the phosphate and well washed out. The clear filtrate now contains iodide of barium; and by double decomposition with carbonate of lithia iodide of lithium is formed. The acid liquid formed by the action of iodine on phosphorus and water, which consists of phosphoric and hydriodic acids, may be neutralized with lime instead of baryta; the method is otherwise the same. In the place of amorphous phosphorus, ordinary phosphorus may be used; the action is then quicker, but also more violent.

Pettenkofer, under Liebig's direction, made experiments in order to see if iodide of potassium could not be advantageously prepared by the above method: one ounce of phosphorus was treated with iodine in the manner described, as long as the iodine dissolved without colouring the liquid; for this $13\frac{1}{2}$ ounces of iodine were necessary. The clear liquid was poured off from a small quantity of amorphous phosphorus which had been formed, and milk of lime, prepared from 8 ounces of burnt lime, added until the liquor was alkaline. After filtration, a hot solution of 9 ounces of crystallized sulphate of potash in 48 ounces of water was added, and the whole allowed to stand six hours. After filtration from the sulphate of lime, the clear liquid was evaporated down to a quart, and a solution of pure carbonate of potash added as long as a precipitate of carbonate of lime was formed. This was filtered; and the clear liquid, when

* Liebig's *Annalen*, February 1862.

evaporated to crystallization, furnished a crop of 13 ounces. The mother-liquor, evaporated to dryness, gave $3\frac{1}{2}$ ounces more of perfectly pure pulverulent iodide of potassium. The theoretical quantity required is $17\frac{1}{2}$ ounces.

V. *Reply to Mr. R. Sabine's "Remarks on the Influence of Traces of Foreign Metals on the Electric Conducting Power of Mercury."*

By A. MATTHIESSEN, F.R.S.; and C. VOGT, Ph.D.*

IN the above (Phil. Mag. June 1862) Mr. Sabine has made some remarks on our paper (Phil. Mag. March 1862) which we cannot leave unanswered. Mr. Sabine says (page 457) "that no formula is given to indicate how the numbers in the seventh columns, headed 'conducting powers calculated,' are obtained."

On referring, however, to our paper (pp. 177, 178), the following statement is made:—"From the foregoing Tables it will be seen that mercury, when alloyed with very minute quantities of another metal, with the exception of zinc, has a greater conducting power; but when alloyed with larger quantities, has a lower conducting power than the mean of the conducting powers of their relative volumes" †.

"The mean conducting powers of their relative volumes" expresses, we think clearly, the method by which the numbers in the seventh columns were calculated. We may be allowed here to add to the passage above quoted, after the words "with the exception of zinc," *gold and silver*; for one of us used in calculating the numbers given in the seventh columns of the gold and silver series, as pointed out by Mr. Sabine (page 458), the values 49.468 and 63.333 for the conducting powers of gold and silver, instead of 494.68 and 633.33 respectively.

Mr. Sabine (page 458) refers to an opinion expressed by Dr. W. Siemens‡, "that the conducting power of a fluid metallic mixture is in proportion to the conducting powers of the two metals in their fluid state at the same temperature." If this opinion be correct, we may deduce the value for the conducting power of the metal alloyed with mercury from the observations made with the amalgams; and we ought also to find that the values so deduced from each observation are the same, for Mr. Sabine has given, in a Table (page 459), only the means of the conducting powers so found. Let us now see how far the above hypothesis will be correct; and in order to do so, we give in Table I. the values deduced from our experiments for the conducting powers of the metals alloyed with mercury.

* Communicated by the Authors.

† Unfortunately "values" has been allowed to stand (*loc. cit.*) as a misprint for "volumes;" we think, however, that no one attentively reading our paper would find much difficulty in making the necessary correction.

‡ Pogg. Ann. vol. cxiii. p. 90.

TABLE I.

Bismuth amalgams.			Lead amalgams.			Tin amalgams.		
Volumes per cent. of bismuth.	Mean conducting power.	Deducted conduct- ing power of bismuth.	Volumes per cent. of lead.	Mean conducting power.	Deducted conduct- ing power of lead.	Volumes per cent. of tin.	Mean conducting power.	Deducted conduct- ing power of tin.
0.069	10.932	42.7	0.0119	10.9185	82.4	0.0186	10.9295	115
0.138	10.9465	37.3	0.0298	10.9285	72.9	0.0465	10.9455	87.3
0.276	10.9785	35.7	0.0596	10.944	67.9	0.0930	10.9775	83.4
0.686	11.0645	33.4	0.119	10.9725	63.4	0.186	11.0415	81.6
1.360	11.1995	32.2	0.238	11.0375	64.5	0.371	11.1171	81.3
			0.593	11.222	63.5	0.922	11.5285	78.0
			1.18	11.495	60.5	1.83	11.792	59.1
			2.33	11.705	45.0	3.59	12.318	50.1
			4.55	11.873	32.1	6.93	13.167	43.5
Zinc amalgams.			Gold amalgams.			Silver amalgams.		
Volumes per cent. of zinc.	Mean conducting power.	Deducted conduct- ing power of zinc.	Volumes per cent. of gold.	Mean conducting power.	Deducted conduct- ing power of gold.	Volumes per cent. of silver.	Mean conducting power.	Deducted conduct- ing power of silver.
0.0190	10.929	110	0.007	10.917	111	0.0130	10.919	80.0
0.0474	10.9507	96.8	0.0176	10.931	130	0.0324	10.9265	61.7
0.0948	10.992	97.4	0.0352	10.9465	114	0.0648	10.948	69.5
0.189	11.077	99.3	0.070	10.9775	107	0.129	10.984	68.2
0.378	11.235	96.9	0.141	11.0315	97	0.259	11.048	64.2
0.940	11.696	94.5	0.351	11.3225	128	0.644	11.200	55.9
1.86	12.450	93.7	0.70	11.5715	105	1.28	11.5665	62.2
3.66	13.566	83.5						
7.06	14.658	64.0						

From the above it is evident that there is a gradual decrement* in the values obtained, and therefore we are not justified in taking, as is done by Mr. Sabine (page 459), the mean of these values and calling them “the conducting powers of the metals in a fluid state” (I.).

Again, in our paper (p. 179) we state that “if to melted bismuth traces of tin or lead be added, a decrement, and, on further addition, an increment in the conducting power will be

* A slight error in the determinations materially affects some of these numbers, owing to the very small amount of metal alloyed with the mercury. Thus the conducting power of the silver amalgam containing 0.025 per cent. of silver was found equal to 10.930 and 10.923; and if we deduce from these the values for the conducting powers of silver, we shall find them equal to 72.5 and 50.9. Again, we find the conducting powers for zinc deduced from the observations made with the amalgam containing 0.025 per cent. equal 93.2, 95.4, and 102.0.

observed" (II. *a*) "In fact, had we determined the conducting power of the melted alloys, we should have obtained similar curves to those found for the conducting power of the alloys in a solid state" (II. *b*). Now, according to the above hypothesis, we ought not to have found a decrement in the conducting power of bismuth on adding traces of lead or tin to it in a liquid state, but an increment; for both lead and tin have a higher conducting power than bismuth, even in a liquid state, according to the values deduced from our experiments by Mr. Sabine.

Again, if we take the mean of the values deduced for the conducting powers of gold and silver, we find for gold 113, and for silver 66·0. Now, when we calculate the conducting power of the gold-silver alloy from these values, we find it equal 90·5 at 13°, the true value being 99·1 at 13° (III.). The calculated value is much too low; but it must be remembered that we have assumed, in order to deduce these values, that the conducting power of mercury in its amalgams remains unaltered, which seems, however, to be exceedingly doubtful. Then, again, if we look at the curves which express the conducting powers of alloys* in a solid state, we find that a great many alloys of lead and silver have nearly the same conducting power. There are also a great many tin-silver alloys whose conducting powers differ very little from each other. Now, if we take the mean values deduced from our experiments, and assume that the conducting powers of the components take part in that of the alloy in the ratio of their relative volumes, we arrive at numbers which really agree approximately with those found by experiment (IV.). In Table II. we give these numbers, and, for the sake of comparison, those calculated in the same manner, using the conducting powers of the metals in their solid state.

TABLE II.

Lead-silver alloys.					Tin-silver alloys.				
Alloy.	Volumes per cent. of lead.	Mean conducting power.	Calculated conducting power.		Alloy.	Volumes per cent. of tin.	Mean conducting power.	Calculated conducting power.	
			Lead = 61·3. Silver = 66·0.	Lead = 52·6. Silver = 633·3.				Tin = 75·5. Silver = 66·0.	Tin = 78·5. Silver = 633·3.
Pb ⁸⁰ Ag	97·79	52·6	61·4	65·4	Sn ¹⁸⁰ Ag	99·28	75·6	75·4	82·5
Pb ²⁰ Ag	94·64	53·6	61·6	83·6	Sn ⁵⁰ Ag	97·47	76·2	75·3	92·5
Pb ⁸ Ag	87·60	56·5	61·9	124·6	Sn ³⁶ Ag	96·52	76·2	75·2	97·8
Pb ⁴ Ag	77·90	59·7	62·3	180·7	Sn ²⁴ Ag	94·87	77·0	75·0	107·0
Pb Ag	63·86	71·0	63·0	262·5	Sn ¹⁸ Ag	93·28	76·9	74·9	115·8
Pb Ag	46·90	77·8	64·8	361·0	Sn ¹² Ag	90·25	76·7	74·6	132·6

* Phil. Trans. 1860, p. 161.

The values given in columns 3 and 4 agree together as well as can be expected when we consider that we have assumed that the conducting power of mercury takes part in that of its amalgams in the ratio of the volumes of mercury employed, and that we have merely taken the mean of the values deduced from the experiments, whereas it is obvious, from Table I., that, if the hypothesis from which we started be correct, the conducting power of a metal deduced from the conducting power of the alloy is no constant value, but varies with the amount of metal added.

With the solid alloys* (those which may be looked upon as a solution of the one metal in the other) we find sometimes,—for instance, with the lead-tin and cadmium-zinc alloys,—that the conducting powers of the components take part in that of the alloy in the ratio of their relative volumes. Sometimes, however, we may assume that the conducting power of one component remains unaltered whilst the other changes—for instance, lead-silver and tin-silver alloys; and lastly, there are cases where the conducting powers of both metals are altered when forming an alloy—for instance, gold-copper, gold-silver, and, in all probability, the amalgams.

The above-mentioned facts seem to indicate that the conducting powers of the component metals of an alloy follow the same laws whether the resulting alloy be a solid or a liquid one, and that Dr. Siemens's hypothesis, from which we started, viz. "*that the conducting power of a fluid metallic mixture is in proportion to the conducting power of the two metals in their fluid state at the same temperature*" [I. II.a], "*and*" (as he continues in his paper already quoted) "*that the reason of the great decrement in the conducting power of solid alloys is only to be looked for in the process of solidification*" [II.b, III. & IV.], is not supported by any facts.

It is our intention to discuss these points more fully in our paper "*On the Influence of Temperature on the Electric Power of Alloys*," which will be shortly ready for publication; for we think we shall now be able, with the help of some new data we are gaining, to calculate from former experiments the parts which the conducting powers of metals take in that of their alloys. We are already able to deduce in a great many cases from the determination of the conducting powers of two metal alloys at 0° and 100°, the percentage amount of the metals composing them.

Mr. Sabine remarks, p. 459, "Dr. Siemens gave the calculated conductivity of fluid silver from three experiments, 8·8, 9·3, and 7·8, taking mercury as unit. These numbers do not differ

* Assuming that the conducting power of an alloy is equal to that of parallel wires of the components forming it.

materially from 5·64" [should be 6·05]*, "the value given in col. B reduced to the same standard, when we consider that the conducting power of solid silver, according to Dr. Matthiessen, is not less than 58·05" [should be 65·9†], "and according to Dr. Siemens 64·78." An important question here is, How is it that the values deduced for the conducting power of silver from its amalgams differ so greatly?

Let us compare the observations made by Dr. Siemens and our own. As the values obtained for zinc differ also, we will include these in Table III., taking as unit the gold-silver alloy (hard drawn) = 100 at 0°.

TABLE III.

Siemens.		Matthiessen and Vogt.		Siemens.		Matthiessen and Vogt.	
Silver per cent. in the amalgam.	Deduced‡ conducting power of silver.	Silver per cent. in the amalgam.	Deduced conducting power of silver.	Zinc per cent. in the amalgam.	Deduced conducting power of zinc.	Zinc per cent. in the amalgam.	Deduced conducting power of zinc.
0·044	96	0·05	69·5	0·76	138	0·5	94·4
0·21	101·5	0·10	68·2	0·825	122	1·0	93·7
0·53	85·1	0·20	64·2	1·520	122	2·0	83·5
		0·50	55·9				

The differences between the two series of values can hardly be due to any errors in observation, but are most probably due to the method employed in making the amalgam and filling the tube with it. And this brings us to the next point in Mr. Sabine's paper; for on this subject he says (page 460) that it is doubtful whether in our experiments filtration of a poorer amalgam into the thermometer-tubes did not take place, and in this manner the values obtained for the richer amalgams appear lower than they really are. He also stated that it would have been better to have analysed the amalgams after each test.

The best answer we can give to these remarks is, to state in a few words the manner in which we made and tested the amalgams. All the amalgams were made in the tube in which they were tested; for we found that when we made the amalgams

* Owing to Mr. Sabine having made a slight error in calculating the values for the conducting power of silver, he has given as the mean value 61·46 instead of 66·0.

† Mr. Sabine has here compared the conducting power of hard-drawn silver at 13° with annealed at 0°. He ought, however, to have taken the value given for annealed silver, viz. 66·6, at 0° (Phil. Mag. Feb. 1861), or, more correctly, that given in a later paper, 65·9 (Pogg. Ann. vol. cxv. p. 363), this number being the mean of several experiments.

‡ Deduced from the observed conducting power of the amalgams, assuming that the hypothesis we started with be correct.

first and then filled the tube with them, as Dr. Siemens did with his zinc amalgams (especially with those which partially solidify on cooling), no concordant results could be obtained; for in manipulating in this manner there is a great probability of the alloy in the tube not having the same composition as that in the cups. Now, if we wished, as Dr. Siemens did, to analyse the contents of the tube after having tested its resistance, it would be necessary to remove the connectors which dip into the cups, and then the cups, in order to empty the tube. But in each of these operations the level of the amalgam in the cups would be disturbed, and so cause, in the case of amalgams which partially solidify on cooling, a flowing of a poorer amalgam into the tube, which on being analysed would contain a less quantity of foreign metal than was contained in the amalgam tested, and in this manner the values deduced for the conducting powers of the metals would be too high. That this really does take place we have often found; for in testing the resistance of a partially solidified amalgam, if one of the connectors be removed and then replaced in the tube, the resistance will have materially altered; but if now both connectors be removed, and the tube heated over a lamp so that the amalgam becomes perfectly fluid, and then retested with the precautions we are about to detail, the original resistance will again be found. We do not find in Dr. Siemens's paper any description of the method he employed to prevent the error just mentioned, nor any indication of the method he used to analyse his amalgams containing very small percentages of foreign metal. In our experiments* the tube was filled with the requisite quantity of mercury, and the other metal added, the tube then carefully heated over a lamp, at the same time allowing the mercury to flow continually from the one arm to the other. When the metal was quite dissolved, the tube was laid in the trough whilst hot, and the copper connectors placed in the wide tubes (these being previously heated when experimenting with amalgams which partially solidify on cooling).

From the form of the tubes, as shown in our paper, page 173, it is evident that, as the thick connectors reached to the bottom of the wide tubes, they, as it were, closed the ends of the thermometer-tubes, and would therefore serve to impede filtration of a poorer amalgam into the tube. The trough being now filled with water, the resistance of the amalgam was tested as soon as it had taken the temperature of the bath. In this manner of experimenting, no filtration of a poorer amalgam into the narrow tubes (which had diameters varying from 1 to 2 millims.) can well take place. It is scarcely probable that, in making the

* For full details of the particulars taken see our paper, *loc. cit.* p. 173.

same amalgam twice in different tubes of different lengths and diameters, the same amount of filtration should occur in both cases. The great concordance in most of the values obtained with amalgams of the same composition proves, we think, conclusively that no such error was made in our experiments: this also proves that the amount of the metal oxidized during the process of heating the amalgams was so small that it has no influence on the values obtained. For, looking at the values deduced for the conducting power of tin from the tin amalgams, published in a former paper* (I.), and on comparing them, as shown in Table IV. with those deduced from the amalgams in our paper (II.), we find them almost identical.

TABLE IV.

I.		I.	
Volumes per cent. of tin.	Deduced conducting power of tin.	Volumes per cent. of tin.	Deduced conducting power of tin.
0·0186	107·6	0·0186	115·0
0·0372	94·1	0·0465	87·3
0·0930	87·1	0·0930	83·4
0·186	80·2	0·186	81·6
0·372	78·8	0·371	81·3
0·926	77·2	0·922	78·0
1·84	62·6	1·83	59·1
3·66	49·4	3·59	50·1
7·19	43·0	6·93	43·5
Mean	75·5	Mean	75·5

With regard to the remarks respecting the analysis of the contents of the tube, we would only state that such analyses would be exceedingly difficult; and it appears to us very doubtful whether they could be made with nearly so great accuracy as the weighing out of the two pure metals and alloying them together.

It is also very doubtful whether, in Dr. Siemens's experiments with the silver amalgams, he did not have in different parts of his tube amalgams of different composition; for they were forced into it by means of a small pressure-pump. We must not overlook one fact of importance, viz. that Mr. Sabine has not taken into consideration whether the alloys partially solidify on cooling or not, but deduces the values for the conducting powers of the metals in a "liquid state" from all the observations.

In conclusion, Mr. Sabine remarks (page 459) that we ought not to have taken into our calculations of the conducting powers of the amalgams the conducting powers of the metals in their solid state with that of fluid mercury. The calculated conduct-

* Phil. Mag. September 1861.

ing powers given by us only serve to show that the amalgams do not conduct electricity in the ratio of the conducting powers of the relative volumes of the components as one might *à priori* have thought. If such calculations were not made, we should sometimes be unable to get an insight into the laws which regulate those properties; thus, for instance, it is only from having calculated the conducting powers in this manner of the solid alloys that we have been led to discover the method of calculating the percentage decrement in the conducting power of an alloy between 0° and 100° ; in fact the calculated conducting powers are necessary to do this. We have already mentioned that it is more than probable that, when metals are dissolved in one another, the part which they then take in the conducting power of the alloy is the same whether the resulting alloy be a solid or a liquid one.

In the foregoing we have assumed, with the hypothesis from which we started, that the amalgams are merely a solution of one metal in the other; but in the case of those of zinc, we have already pointed out that with the richer ones this is not the case; and as we at present know so little regarding the chemical nature of the amalgams, it seems to us that Dr. Siemens's hypothesis, based only on the determination of six amalgams, viz. three with zinc and three with silver, was rather premature. That chemical combinations exist in the amalgams is more than probable from the experiments of Joule*, Crookewitt†, and others; so that it is clearly wrong to try to deduce laws respecting the parts the components take in the properties of the alloys from a few observations, where we have no knowledge as to their chemical nature.

11 Torrington Street,
June 1862.

VI. On a New Species of Differential Equations.

By JAMES COCKLE, M.A., F.R.A.S., F.C.P.S. &c.‡

IN a paper§ published in this Journal for May 1861, I announced and demonstrated that from any algebraic equation of the degree n , whereof the coefficients are functions of a variable, there may be derived a linear differential equation, of the order $n-1$, soluble by means of the given algebraic equation. Applying this theorem to certain trinomial equations, Mr. Harley has discovered new primary forms of linear differential equations||,

* Chem. Gaz. 1850, p. 329.

† *Ann. der Chem. und Pharm.* vol. lxxviii. p. 289.

‡ Communicated by the Author.

§ "On Transcendental and Algebraic Solution," S. 4. vol. xxi. p. 379.

|| See the 'Proceedings of the Literary and Philosophical Society of

that is to say, forms not recognized as primary in Dr. Boole's theory. From the general cubic I have deduced results, a concise statement of which is subjoined. Differentiation with respect to the independent variable I denote by accents, in conformity with the usage of Professor De Morgan.

Starting with the cubic

$$ax^3 + 3bx^2 + 3cx + d = 0, \quad . \quad . \quad . \quad . \quad (1)$$

I write

$$\begin{aligned} a &= ac - b^2, \\ b &= ad - bc, \\ A &= -ab^3 + 3b \cdot 2a \cdot b^2 - 3c \cdot 2^2 a^2 \cdot b + d \cdot 2^3 a^3, \\ \beta &= ba' - ab', \\ \gamma &= ca' - cb', \\ \delta &= \frac{1}{3}(da' - ad'), \\ B &= 2aa(2ba - ab), \\ C &= 12b^2a^2 - 8abab + a^2b^2, \\ D &= (12bc - 4ad)a^2 - 6b^2ab + abb^2, \\ E &= B\delta + C\gamma + D\beta - \frac{3b}{a}(B\gamma + C\beta) + \frac{9b^2 - 3ac}{a^2}B\beta, \\ F &= C\delta + D\gamma - \frac{3c}{a}(B\gamma + C\beta) + \frac{9bc - ad}{a^2}B\beta, \\ G &= D\delta - \frac{d}{a}(B\gamma + C\beta) + \frac{3bd}{a^2}B\beta, \\ H &= AE' - \frac{1}{a}(6bE^2 - 3aEF), \\ I &= AF' - \frac{1}{a}(6cE^2 - 2aEG - aF^2), \\ J &= AG' - \frac{1}{a}(2dE^2 - aFG); \end{aligned}$$

and, eliminating x^2 between

$$Ax' = Ex^2 + Fx + G,$$

and

$$A^2x'' + AA'x' = Hx^2 + Ix + J,$$

I find

$$Kx'' + Lx' + Mx = N, \quad . \quad . \quad . \quad . \quad (2)$$

wherein

$$\begin{aligned} K &= A^2E, \\ L &= AEA' - AH, \\ M &= FH - EI, \\ N &= EJ - GH. \end{aligned}$$

Expressing A, B, C, and D in terms of a, b, c , and d , I find

$$A = -a^4d^3 + 9a^3bcd^2 - 4a^3c^3d - 6a^2b^3d^2 - 15a^2b^2c^2d + 12a^2bc^4 \\ + 24ab^4cd - 17ab^3c^3 + 6b^5c^2 - 8b^6d,$$

$$B = -2a^4cd + 2a^3b^2d + 6a^3bc^2 - 10a^2b^3c + 4ab^5,$$

$$C = a^4d^2 - 10a^3bcd + 8a^2b^3d + 21a^2b^2c^2 - 32ab^4c + 12b^6,$$

$$D = a^3bd^2 - 4a^3c^2d + 12a^2bc^3 + 2ab^4d - 17ab^3c^2 + 6b^5c.$$

Every root of the cubic (1) satisfies the linear differential equation (2), which is of the second order. Therefore among the integrals of (2) the roots of (1) are included.

4 Pump Court, Temple, London,
June 14, 1862.

VII. *On some Applications of a Theorem relating to Parallel Surfaces.* By the Rev. WILLIAM ROBERTS*.

THEOREM.—*In the equation of a surface parallel to a given one, let x, y, z be replaced by $\frac{1}{2}x, \frac{1}{2}y, \frac{1}{2}z$, and let $\frac{1}{2}\sqrt{x^2 + y^2 + z^2}$ be written instead of k , the constant length taken on the normals of the given surface; the surface represented by the equation which results from these substitutions will be the envelope of planes passing through the several points of the given surface, and perpendicular at each point to the radius vector drawn to the point from the origin of coordinates.*

This theorem, which I have already published under a slightly different enunciation (*Comptes Rendus*, November 14, 1859), may be demonstrated as follows:—

Let x', y', z' be the coordinates of a point on the given surface: the parallel surface will be the envelope of spheres represented by the equation

$$(x - x')^2 + (y - y')^2 + (z - z')^2 = k^2,$$

or

$$2xx' + 2yy' + 2zz' - x'^2 - y'^2 - z'^2 = x^2 + y^2 + z^2 - k^2.$$

Now suppose the eliminations requisite for finding this envelope performed, it is plain that the result, when transformed by the substitutions mentioned in our theorem, will be identical with the equation representing the envelope of the planes,

$$xx' + yy' + zz' = x'^2 + y'^2 + z'^2,$$

which pass through the extremities of the radii vectores (from the origin) of the given surface, and are perpendicular to them.

The surface envelope of planes perpendicular to the radii vectores of a surface and passing through their extremities has been treated of by Mr. T. A. Hirst in a very valuable memoir,

* Communicated by T. A. Hirst, Esq.

published in Tortolini's *Annali*, vol. ii.*, under the name of the first negative derived surface—a name suggested by its analogy to the first negative derived curve, so called by me in a paper published many years ago in Liouville's *Journal de Mathématiques*, sér. 1. vol. x.

If we bear in mind that the parallel to a given surface is connected absolutely with it, we see at once that, if its equation be obtained (which in some cases may be done with comparative facility by a judicious selection of the origin), we shall have the equation of the first negative derived from the given surface in reference to any origin whatever, a problem the direct solution of which might be a matter of considerable difficulty; for we have only to transfer the equation of the parallel surface to any point arbitrarily chosen as origin, and then to make in it the substitutions prescribed by our theorem. Again, it follows obviously that if we put $\frac{1}{2}x$, $\frac{1}{2}y$, $\frac{1}{2}z$ for x, y, z , and $k + \frac{1}{2} \sqrt{x^2 + y^2 + z^2}$ instead of k in the equation of the parallel, the resulting equation will be that of the first negative derived from the parallel in reference to the origin; and, as we have just remarked, we can also deduce from the equation of the parallel that of the first negative of the parallel in reference to any origin whatever. It may be interesting to verify the theorem by applying it to the obvious case of concentric spheres. The parallel surface to the sphere

$$(x-\alpha)^2 + (y-\beta)^2 + (z-\gamma)^2 = \delta^2 \quad . \quad . \quad . \quad (I.)$$

is

$$(x-\alpha)^2 + (y-\beta)^2 + (z-\gamma)^2 = (\delta + k)^2.$$

Hence the first negative of the sphere (I.) in reference to the origin is

$$(x-2\alpha)^2 + (y-2\beta)^2 + (z-2\gamma)^2 = (2\delta + \sqrt{x^2 + y^2 + z^2})^2,$$

or

$$\delta \sqrt{x^2 + y^2 + z^2} = \alpha^2 + \beta^2 + \gamma^2 - \delta^2 - \alpha x - \beta y - \gamma z,$$

a surface of revolution of the second degree, the origin being a focus—a well-known result.

The discovery, in 1859, of the theorem which is the subject of this note, naturally led me to investigate the equation of the surface parallel to the ellipsoid, an equation which, as I believe, had not at that time been given—as I saw that it would enable me to find the equation of the first negative of the ellipsoid for any origin, a problem which had shortly before received a most elegant solution from Mr. Cayley for the case of the centre as origin. I succeeded in obtaining a solution, which I communicated to Mr. Hirst in December 1859. Although inferior in

* See also an abstract of the memoir in *Quart. Journ. of Math.* vol. iii.

point of symmetry to the solution since given by Mr. Cayley, I think it worth mentioning, and therefore subjoin it.

Consider any one of the circular sections of the surface, and suppose the annular surface described which is the envelope of a sphere with constant radius, whose centre moves along the circumference of the section: it is evident that the parallel surface will be the envelope of all the annular surfaces belonging to the circular sections of one series. Let the axis of x be the trace of one of the central circular sections on the plane of the greatest and least axes, that of z the diameter in this plane perpendicular to the axis of x , and that of y the mean axis of the surface. Transfer the origin, for the moment, to the middle point ($x'z'$) of a chord parallel to the axis of x . If 2δ be the length of this chord, the equation of a sphere with radius k , the centre of which lies on the circumference of the circular section whose trace is the chord, is

$$(x - \delta \cos \phi)^2 + (y - \delta \sin \phi)^2 + z^2 = k^2;$$

and the equation of the envelope of these spheres, ϕ being a parameter, or of the annular surface round this circular section, will be

$$4\delta^2(x^2 + y^2) = (x^2 + y^2 + z^2 - k^2 + \delta^2)^2;$$

or, when we transfer the origin back again to the centre,

$$4\delta^2 \{ (x - x')^2 + y^2 \} = \{ (x - x')^2 + y^2 + (z - z')^2 - k^2 + \delta^2 \}^2.$$

But it may be easily shown that

$$z' = lx', \quad \delta^2 = m + nx'^2,$$

l, m, n being absolute constants depending only on the axes of the ellipsoid. Hence the equation of the annular surface, involving a single parameter x' , may be written

$$\begin{aligned} & 4 \{ m + nx'^2 \} \{ x^2 + y^2 - 2xx' + x'^2 \} \\ & = \{ x^2 + y^2 + z^2 - k^2 + m - 2(x + lz)x' + (1 + n + l^2)x'^2 \}^2. \end{aligned}$$

Since the parameter x' enters in the fourth degree, the equation of the envelope or of the parallel surface may be written in the form $S^3 = T^2$, according to the well-known type of the discriminant of a quartic function, due to Messrs. Boole and Cayley. The solution of the problem, to find the equation of the surface parallel to an ellipsoid, involves, we repeat, that of the two following problems:—

1. To find the equation of the surface which is the envelope of planes passing through the points of an ellipsoid, and perpendicular at each point to the radius vector drawn from any fixed point whatever.

2. To find the equation of the surface which is the envelope

of planes passing through the points of a surface parallel to an ellipsoid, and perpendicular at each point to the radius vector drawn to the point from any fixed origin.

We now proceed to another application of the theorem. M. Liouville has shown (*Journal de Mathématiques*, January 1851) that the system of parallel surfaces, which have for loci of their centres of curvature a determinate pair of confocal surfaces of the second order (represented by the constants α and β), is given by the equation in elliptic coordinates

$$\int P d\rho + \int M d\mu + \int N d\nu = C, \quad . \quad . \quad . \quad (II.)$$

where

$$P^2 = \frac{(\rho^2 - \alpha^2)(\rho^2 - \beta^2)}{(\rho^2 - b^2)(\rho^2 - c^2)}, \quad M^2 = \frac{(\alpha^2 - \mu^2)(\mu^2 - \beta^2)}{(c^2 - \mu^2)(\mu^2 - b^2)},$$

$$N^2 = \frac{(\alpha^2 - \nu^2)(\beta^2 - \nu^2)}{(b^2 - \nu^2)(c^2 - \nu^2)}.$$

If C' be another value of C , $C - C'$ will be the portion of any common normal to the two parallel surfaces which correspond to these two values of the constant intercepted between them. Hence the first negative of (II.), in reference to the centre, or, more correctly, the surface similar to it by doubling its radii vectores, will have for equation

$$\int P d\rho + \int M d\mu + \int N d\nu = C + \sqrt{\rho^2 + \mu^2 + \nu^2 - b^2 - c^2},$$

as appears at once if we remember that

$$\rho^2 + \mu^2 + \nu^2 = x^2 + y^2 + z^2 + b^2 + c^2.$$

If the loci of the centres of curvature be supposed to degenerate into the focal conics, which is expressed by making $\alpha = c$, $\beta = b$, the parallel surfaces become algebraic, and their equation in elliptic coordinates takes the very simple form

$$\rho + \mu + \nu = C. \quad . \quad . \quad . \quad (III.)$$

This surface was discovered long ago by M. Charles Dupin, and he gave it the name of *cyclide*. It forms the subject of a variety of interesting articles in the *Correspondance de l'Ecole Polytechnique*. M. Mannheim has lately published a complete and elegant discussion of its properties in the nineteenth volume of the *Nouvelles Annales de Mathématiques**.

The surface similar, by doubling its radii vectores, to the first negative of the cyclide (III.), is given by the equation in elliptic

* In mentioning this periodical, I cannot refrain from joining in the general expression of regret which the recent death of its editor, M. Terquem, has called forth. Even those who knew him only by corresponding with him feel as if they had lost an esteemed personal friend.

coordinates,

$$\rho + \mu + \nu = C + \sqrt{\rho^2 + \mu^2 + \nu^2 - b^2 - c^2},$$

or

$$\rho\mu + \rho\nu + \mu\nu - C(\rho + \mu + \nu) + \frac{1}{2}(C^2 + b^2 + c^2) = 0. \quad (\text{IV.})$$

The equation (III.) of the cyclide, transformed into an equation between x, y, z , becomes

$$\{x^2 + y^2 + z^2 + b^2 + c^2 - C^2\}^2 = 4\{(b^2 + c^2)x^2 + c^2y^2 + b^2z^2 + 2bcCx + b^2c^2\}.$$

This equation representing a system of parallel surfaces, if we regard C as a parameter, we at once deduce the first negative of any one of them corresponding to a particular value of C by applying our theorem. Accordingly let us write $\frac{1}{2}x, \frac{1}{2}y, \frac{1}{2}z$ for x, y, z , and $C + \frac{1}{2}\sqrt{x^2 + y^2 + z^2}$ for C , and we shall find for the equation of the first negative in reference to the centre of the elliptic system as origin,

$$\{(C^2 - b^2 - c^2)x^2 + (C^2 - c^2)y^2 + (C^2 - b^2)z^2 - 4bcCx + (c^2 - b^2)^2 - 2C^2(b^2 + c^2) + C^4\}^2 = 4\{C^3 - b^2C - c^2C - bcx\}^2\{x^2 + y^2 + z^2\},$$

as we might also have found by transforming (IV.) out of the elliptic system. I have already published this result in a note presented to the French Academy of Sciences (*Comptes Rendus*, December 16, 1861).

We now proceed to show how, from the equation of a surface parallel to a given one, we can deduce the equation of a parallel to the surface derived from the given by the method of reciprocal radii vectores. Let x', y', z' be the coordinates of a point on the given surface, and m^2 the constant rectangle formed by the coincident radii vectores of this surface and its inverse. The parallel surface to the inverse is the envelope of the spheres represented by the equation

$$\left\{x - \frac{m^2x'}{x'^2 + y'^2 + z'^2}\right\}^2 + \left\{y - \frac{m^2y'}{x'^2 + y'^2 + z'^2}\right\}^2 + \left\{z - \frac{m^2z'}{x'^2 + y'^2 + z'^2}\right\}^2 = k^2.$$

But this equation may be written

$$(\theta x - x')^2 + (\theta y - y')^2 + (\theta z - z')^2 = \theta^2 k^2,$$

where

$$\theta = \frac{m^2}{x^2 + y^2 + z^2 - k^2};$$

whence it appears that if, in the equation of the parallel to a given surface, we replace x, y, z , and k respectively by $\theta x, \theta y, \theta z, \theta k$, we shall have the equation of the parallel to the inverse of

the given surface. It is evident that the direct application of our theorem in order to obtain from the equation of the parallel to a given surface that of the first negative derived from its inverse, would introduce infinite terms; it will be necessary therefore to take the expression $x^2 + y^2 + z^2 - k^2$ out of all the denominators before applying our theorem for this purpose.

To verify our last result in conjunction with the first theorem, let us seek to deduce the first negative of a sphere from the equation of a plane parallel to a given one. The parallel to the plane $z = \alpha$ is $z = \alpha + k$. Hence the parallel to the sphere which is the inverse of $z = \alpha$ has for equation

$$x^2 + y^2 + z^2 - k^2 = \frac{m^2}{\alpha} (z - k);$$

and to find the first negative from this sphere, we have only to put $\frac{1}{2}x$, $\frac{1}{2}y$, $\frac{1}{2}z$ for x , y , z , and $k + \frac{1}{2} \sqrt{x^2 + y^2 + z^2}$ for k . This gives the equation

$$(m^2 + \alpha k) \sqrt{x^2 + y^2 + z^2} = m^2 z + \alpha k^2 - 2m^2 k,$$

which represents a surface of revolution of the second order, the origin being one of the foci, as it ought to do.

We are now in a position to solve the problem of finding the equation of the parallel to the surface of the fourth order which is derived from an ellipsoid by the method of reciprocal radii vectores drawn from any point whatever. It will only be necessary to place the origin at any point we choose, and to make in the equation of the parallel to the ellipsoid, referred to this origin, the substitutions indicated above. We are also led to the solution of the two following problems:—

1. To find the equation of the surface parallel to the first positive derived (in reference to any origin) of an ellipsoid.

2. Being given an ellipsoid, and two fixed points situated anywhere: let P be the foot of a perpendicular dropped from either of the fixed points on any tangent plane to the ellipsoid; it is required to determine the surface which is the envelope of the plane passing through P, and perpendicular to the line joining P with the other of the fixed points.

For the inverse of the locus of P is an ellipsoid; hence the equation of the parallel to this locus can be found, and by our first theorem its first negative in reference to any origin may be deduced from this equation.

All the foregoing results apply, *mutatis mutandis*, to the case of curves. As an application of them, I subjoin the following transformation of the equation of the curve parallel to the ellipse, into that of the parallel to the first positive derived from the ellipse, referred to the centre, and whose equation is

$$a^2 x^2 + b^2 y^2 = (x^2 + y^2)^2.$$

I adopt the form of the equation of the parallel given by M. Catalan (*Nouvelles Annales*, vol. iii. p. 555); and by writing in it $\frac{1}{a}, \frac{1}{b}$ for a, b , and $\frac{x}{x^2+y^2-k^2}, \frac{y}{x^2+y^2-k^2}, \frac{k}{x^2+y^2-k^2}$ for x, y , and k , I find for the equation of the parallel to

$$a^2x^2 + b^2y^2 = (x^2 + y^2)^2,$$

$$\begin{aligned} & \{a^2b^2 - (a^2 + b^2)(x^2 + y^2 - k^2)\}^2 \{a^2x^2 + b^2y^2 - (a^2 + b^2)k^2 - (x^2 + y^2 - k^2)^2\}^2 \\ & + 4k^2 \{x^2 + y^2 - k^2\} \{a^2b^2 - (a^2 + b^2)(x^2 + y^2 - k^2)\}^3 \\ & - 27a^4b^4k^4(x^2 + y^2 - k^2)^2 + 18a^2b^2k^2 \{x^2 + y^2 - k^2\} \{a^2b^2 \\ & - (a^2 + b^2)(x^2 + y^2 - k^2)\} \{a^2x^2 + b^2y^2 - (a^2 + b^2)k^2 - (x^2 + y^2 - k^2)^2\} \\ & + 4a^2b^2 \{a^2x^2 + b^2y^2 - (a^2 + b^2)k^2 - (x^2 + y^2 - k^2)^2\}^3 = 0. \end{aligned}$$

Now, put $x^2 + y^2 = k^2$ in this equation, and it becomes

$$a^4b^4(a^2y^2 + b^2x^2)^2 - 4a^2b^2(a^2y^2 + b^2x^2)^3 = 0.$$

Dividing by $a^2b^2(a^2y^2 + b^2x^2)^2$, and writing $\frac{1}{2}x, \frac{1}{2}y$ for x, y , we get for the first negative of $a^2x^2 + b^2y^2 = (x^2 + y^2)^2$,

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} = 1,$$

as indeed is evident. We thus obtain an interesting verification of our different theorems.

Again, taking from Salmon's 'Higher Plane Curves,' p. 279, the equation in elliptic coordinates of the involute of an ellipse, namely,

$$\int \sqrt{\frac{\mu^2 - \mu_1^2}{\mu^2 - c^2}} d\mu \pm \int \sqrt{\frac{\mu_1^2 - \nu^2}{c^2 - \nu^2}} d\nu = C,$$

we have for the first negative of the involute the equation

$$\int \sqrt{\frac{\mu^2 - \mu_1^2}{\mu^2 - c^2}} \pm \int \sqrt{\frac{\mu_1^2 - \nu^2}{c^2 - \nu^2}} = C + \sqrt{\mu^2 + \nu^2 - c^2}.$$

Mr. Salmon has derived the equation of the surface parallel to an ellipsoid from the condition that a sphere should touch the ellipsoid. The condition, in general, that a sphere should touch a given surface is expressed by a relation between the coordinates of the centre of the sphere, its radius, and the constants in the equation of the given surface. If in this relation we suppose the radius of the sphere to be constant, and regard the coordinates of the centre as variable, we have the equation of the parallel surface. It is evident also that the above relation will give the locus of the centre of a sphere tangent to the given surface, and the radius of which is a given function of the coordinates of the centre. If we suppose that the equation of the parallel surface has been obtained, such a locus will be had by

making in it $k = F(x, y, z)$, F denoting a function of given form. We can in this way prove the theorem which is the subject of this note. Let O be the origin, P a point on the given surface, P' the corresponding point on the first negative. By the fundamental property of derived surfaces, noticed in Mr. Hirst's memoir, the normal to the surface at P will meet the line OP' in its middle point M . Hence the locus of M is similar to the first negative, and is had by bisecting the radii vectores of this latter surface. But M is evidently the centre of a sphere touching the given surface, and the radius of which is equal to the distance of M from the origin of coordinates. It follows, therefore, that the locus of M is found by making, in the equation of the parallel surface, $k^2 = x^2 + y^2 + z^2$.

Trinity College, Dublin,
June 13, 1862.

With reference to the concluding part of Mr. Roberts's very interesting paper, it may be noticed that his second theorem relating to the parallel of the inverse surface admits of a similar proof. In fact, if the inverse surface is to be touched by a sphere, whose centre is at x, y, z , and whose radius is k , the primitive surface will necessarily be touched by the inverse of this sphere whose centre will clearly be at the point $\theta x, \theta y, \theta z$, and whose radius will be θk , where θ has the value given above. These last values, then, when put for x, y, z, k , will satisfy the condition that the primitive surface is touched by a sphere, in other words, will satisfy the given equation of the parallel to the primitive, and transform this equation to that of the parallel to the inverse.—
T. A. H.

VIII. On the Preparation of the Rubidium Compounds.

By R. BUNSEN*.

CERTAIN varieties of lepidolite occur which, as they contain more than 1 per cent. of rubidium, may with advantage be employed for the preparation of the salts of this new alkaline metal, as well as for that of the lithia compounds.

I am indebted to my friend Dr. O. Struve, of Leipzig, for 15 kilogrammes of a residual salt derived from the preparation of lithia, which is so rich in chloride of rubidium that it may thus be prepared by the pound. This residual salt consists of the chlorides of sodium, potassium, and rubidium, together with small quantities of chloride of cæsium, and traces of chloride of

* Translated by Professor Roscoe from *Ann. der Chem. und Pharm.* vol. cxxii. p. 347.

strontium. In order to obtain the average composition of this somewhat unequally mixed mass, 550 grms. of it were dissolved in 1975 grms. of water, and this solution analysed as follows:—

(a) 7·1057 grms. of the liquid yielded 1·4065 gm. of mixed anhydrous chlorides. Hence the original saline residue contained in 100 parts—

Water	10·92
Chlorine compounds	89·08
	<hr/> 100·00

(b) The amounts of chloride of potassium and chloride of rubidium can be found by precipitating the double chlorides of these metals and platinum, and by weighing the platinum which the double salts yield on reduction in a current of hydrogen. This method is, however, open to the objection that commercial platinum, unless purified by a series of long and tedious processes, is quite unfit for any accurate quantitative estimation, as it invariably contains impurities which cause its atomic weight to vary several per cent. It is therefore much easier and simpler to wash out the mixed chlorides of potassium and rubidium from the reduced mass, then to weigh them, and to determine their yield of chlorine as chloride of silver.

If we call the weight of the chloride of potassium x , and the weight of the chloride of rubidium y , A being the sum of the weights of the mixed chlorides, and B the weight of the chloride of silver obtained, we have

$$\frac{\text{Ag} + \text{Cl}}{\text{K} + \text{Cl}} = a, \text{ and } \frac{\text{Ag} + \text{Cl}}{\text{Rb} + \text{Cl}} = b;$$

hence

$$x = \frac{bA - B}{b - a}, \text{ and } y = A - x,$$

or

$$x = 1·3601 B - 1·6143 A. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

4·0017 grms. of the solution, corresponding to 0·7921 gm. of the dry chlorides, gave 0·4723 gm. chlorides of rubidium and potassium = A ; and these yielded 0·7787 = B gm. of chloride of silver. 89·08 parts of the above chlorides therefore contain 33·37 parts chloride of potassium, and 19·75 parts chloride of rubidium. This latter salt was found, on examination with the spectrum apparatus, to contain a little cæsium.

It may not be superfluous to mention that the formula (1) is not applicable in the case of very slight differences in the values of x and y , or for very small values of A . Hence it is advisable to calculate the probable error of x and y for each of the values

of A and B employed: if we put $\xi = \frac{x}{A}$, and $\eta = \frac{y}{A}$, and if we suppose that the error (δA) made in the determination of A and

B is in both cases the same, then we have $\xi = \frac{b - \frac{B}{A}}{b - a}$, and the probable error of ξ

$$(\delta \xi) = \frac{1}{b - a} \cdot \sqrt{1 + \left(\frac{B}{A}\right)^2 \cdot \frac{dA}{A}}. \quad . \quad . \quad . \quad (2)$$

If in the preceding example the error in the determination of A and B in each case amounted to 0.001 gm., the most probable inaccuracy in the determination of the chlorides of rubidium and caesium in 1 gramme of both does not exceed 0.0055 gm.

(c) I have also employed an indirect method for the estimation of the chloride of lithium, as the direct separation of this metal from those of the other alkalis by extraction of the chloride or sulphate with a mixture of alcohol and ether, is as inaccurate as the determination by means of phosphate of sodium.

8.3300 grms. of the anhydrous substance was extracted four consecutive times with a mixture of ether and alcohol, the residue obtained by evaporating the alcoholic liquid was again treated with ether and alcohol, and the residue obtained by evaporating this second extract was heated until fusion commenced. This mass weighed 0.0628 gm., and, when dissolved in water and treated with nitrate of silver, yielded 0.1650 gm. of chloride of silver. The filtrate from the precipitate of chloride of silver was carefully freed from silver by hydrochloric acid, and the potassium precipitated as the double platinum-salt; 0.0278 gm. of this double salt was thus obtained, corresponding to 0.0088 gm. chloride of potassium and 0.0163 chloride of silver. The salt weighing 0.0628 gm. contains y of chloride of sodium, x of chloride of lithium, and 0.0088 of chloride of potassium; if we subtract the weight of the latter salt, we have $A = 0.054$ gm. for the sum of the weights of x and y ; and for the weight of chloride of silver which these two chlorides yield, we have $B = 0.1487$.

If the corresponding values of a and b for the chlorides of lithium and sodium are substituted in formula (1), we have

$$x = 1.0823 B - 2.6525 A,$$

and hence $x = 0.0177$; so that 89.08 parts of the dried substance contain 0.190 part of chloride of lithium.

If the chloride of sodium be estimated by difference, we obtain the following as the percentage composition of the saline mass:—

Chloride of sodium	35·77
Chloride of potassium	33·37
Chloride of rubidium	19·75
Chloride of lithium	0·19
Chloride of cæsium (present in too small quantities for estimation).	
Chloride of strontium	traces
Water	10·92
	<hr/> 100·00

This source of rubidium accordingly contains 3 ounces of chloride of rubidium per pound.*

The preparation of the chloride of rubidium is effected best as follows: a kilogramme of the salt is dissolved in 2·5 kilogs. of water, and precipitated in the cold by a solution of about 30 grms. of platinum in aqua regia. As soon as the yellow precipitate has sufficiently subsided, the supernatant liquid is poured into a large beaker-glass, and the precipitate brought into a basin in order to boil it out twenty-five consecutive times with small quantities of water. For this purpose about 1·5 kilog. of water, in all, may be used; and the operation is best performed in a large platinum capsule, from which the volume of water used for boiling out the precipitate is poured boiling-hot into the original decanted liquid. A new precipitate then falls down from the liquid, now amounting to about 4 kilogs. The liquid poured off from this precipitate is next boiled down until, on adding it again to the precipitate, the volume is not greater than it was at the commencement of the operation. The platinum obtained by reducing the purified precipitate in hydrogen is dissolved in aqua regia, and this added to the boiled-down liquid; the precipitate which falls, as well as the supernatant liquid, is then under the same conditions as it was at the beginning of the preparation. Precipitate and liquid can therefore be treated again in the manner just described.

When these operations have been repeated seven or eight times, the greater part of the chloride of rubidium contained in the original kilogramme of salt has been extracted. Each of the seven or eight platinum-precipitates is dried at 100° C. in the dish in which it was boiled, then brought into a glass tube and reduced in a current of hydrogen at a *temperature below a red heat*, and below the melting-point of the chloride of rubidium. By digesting the black reduced mass with warm water, the soluble chloride of rubidium is easily extracted from the platinum, which, as

* This salt can now be obtained from Dr. Struve's mineral-water manufactory in Leipzig at a price of 6 thalers the kilogramme, or about 9s. per pound.

has been already mentioned, when dissolved in aqua regia, may at once be used for the next precipitation. In this way, by using only 30 grms. of platinum, which can be regained at the end of the operation with very slight loss, more than a quarter of a pound of chloride of rubidium can be obtained, containing only from about 3 to 4 per cent. of the chlorides of potassium and caesium. In order to separate these impurities, the 36 grms. of the salt and the 30 grms. of platinum as chloride are each mixed with a kilogramme of water, and the solutions mixed when boiling. On cooling down to 40°C ., a yellow sandy precipitate is deposited, and this is washed by decantation with water of the temperature of about 40°C . The chloride of rubidium obtained from this precipitate by reduction in a current of hydrogen is dissolved in water, and the chloride of potassium separated by a repetition of the foregoing operation, until a small quantity of the salt examined in the spectrum apparatus shows no trace of the red potassium line.

The salt likewise contains a small quantity of chloride of caesium, the spectrum of which, however, is not visible until the potassium has been removed. For the purpose of separating the caesium, the chlorides are converted into sulphates, and the sulphuric acid separated by the addition of a slight excess of baryta water, the resulting hydrated oxide of rubidium being evaporated to dryness with carbonate of ammonium in a silver basin. The carbonate of rubidium, which is separated by previous filtration from a trace of carbonate of barium, is completely dried, and the dry powder extracted twenty to thirty times with boiling absolute alcohol. The carbonate of caesium is thus dissolved, leaving behind pure carbonate of rubidium.

The salt is pure as soon as no trace of the caesium lines are seen in the spectrum. The few grammes of chloride of caesium contained in it may be obtained by the evaporation of the alcoholic solution.

It is not advisable to separate the chlorides of sodium and potassium by preliminary crystallization of the original salt. This is seen by the following experiment made by M. Wander:—200 grms. of the salt were dissolved in water and the solution evaporated by boiling; as soon as about one-third of the salt had separated out in the form of crystals, the mother-liquor was poured off and evaporated to dryness. The residue, completely dried, weighed 32.955 grms.; 2.7975 grms. of this salt, when treated with chloride of platinum, yielded 2.0465 grms. = A of a mixture of the chlorides of rubidium and potassium, which, precipitated with nitrate of silver, gave 2.724 grms. = B of chloride of silver. Hence the salt consists of

Chloride of rubidium . . .	59·80
Chloride of potassium . . .	13·35
Chloride of sodium } . . .	26·85
Chloride of lithium } . . .	
	100·00

Although the salt remaining in the mother-liquor contains, as the preceding analysis shows, about 60 per cent. of chloride of rubidium, a preliminary separation by crystallization is not advisable, as the salt which crystallizes out contains nearly half the total quantity of chloride of rubidium, thus rendering necessary a repetition of the complete series of platinum precipitations, and tedious washings with boiling water.

Heidelberg, March 1862.

IX. *Observations upon a Paper by M. De la Rive "On the Auroræ Boreales," in the Supplement to the Philosophical Magazine for June 1862. By Professor POTTER, A.M.**

IN his paper entitled "Further Researches on the Auroræ Boreales, and the Phenomena which attend them," M. De la Rive assumes that the meteor is an atmospheric and terrestrial result only. If he had first studied the locality of the phenomena, I have no doubt he would have given us a valuable paper, from his great knowledge of the science of electromagnetism; but he unfortunately follows the old Aristotelian view, that the meteor is terrestrial and atmospheric, and ignores the scientific principle that inaccessible heights and distances can be only measured by parallaxic observations with the aid of trigonometry.

The reasonings and measurements of Halley†, Cavendish‡, Bergman, Dalton§, and others have given the locality of the meteor as beyond the earth's atmosphere, and its height from 52 miles the lowest, calculated by Cavendish, to very great heights.

At the Meeting of the British Association at Cambridge in 1833, after I had read a paper on the height of the Aurora of March 21st, 1833||, and a discussion which followed, a committee was appointed to direct observers, collect observations, and settle the discordant opinions on the subject of the locality of the meteor. Only one hurried meeting of a few of the committee was held at Cambridge, at which Dalton was not present;

* Communicated by the Author.

† Phil. Trans. for 1716.

‡ Ibid. 1790.

§ Meteorology, 2nd edition, p. 227; and Phil. Trans. 1828.

|| See Phil. Mag. for December 1833.

but I informed him afterwards of what had taken place. Two very fine displays of the meteor occurred shortly after, at the end of September and near the middle of October; and regular observations were obtained over a great extent of country, of which the calculations by myself are published in the Cambridge Transactions for 1846.

In the spring and summer of 1834, having to recover in haste my classical studies in preparation for commencing residence as an undergraduate of Queen's College, Cambridge, in October of that same year, I did not attend the Meeting of the British Association at Edinburgh (nor since, except for a day or two at Cambridge in 1845), but on calling upon my old friend and former private tutor in chemistry, Dr. Dalton, to bid him good-bye on my going up to Cambridge, we had a conversation very interesting to both upon the Aurora-Borealis committee which had been appointed in 1833. He informed me that he had made inquiries respecting meetings of the committee which ought to have been held at Edinburgh, but could learn nothing on the subject, and only heard from Sir D. Brewster, "Oh, you have it all your own way," meaning that the advocates of the atmospheric locality of the meteor had retired from the discussion.

Was this a conclusion of the discussion of the subject worthy of a national association for the advancement of science? Was this a scientific morality that could be defended? namely, the suppression of a committee before it had made its report, because those who had assumed the management of the Association had advocated an untenable hypothesis, and to this day leave the scientific world floundering in discussions of atmospheric auroræ boreales which, it has been demonstrated, can have no existence.

The hypothesis which I have published elsewhere, that the auroræ boreales are caused by the earth's electro-magnetism acting upon masses of very rare vapours, of like constituents to the meteoric stones and vaporous comets moving in the planetary spaces under the laws of gravitation, and coming near the boundaries of our atmosphere, is, I believe, the only tenable one.

X. Notices respecting New Books.

Researches on the Solar Spectrum, and the Spectra of the Chemical Elements. By G. KIRCHHOFF, Professor of Physics in the University of Heidelberg. Translated, with the Author's sanction, from the Transactions of the Berlin Academy for 1861, by HENRY E. ROSCOE, B.A., Professor of Chemistry in Owens College, Manchester. Macmillan and Co. 1862.

ALL who are interested in the study of the spectrum will be glad to learn that this important memoir by Professor Kirchhoff

has been placed within easy reach of English readers. Great is the advantage of referring to original writings in all matters of science and exact information. Yet the student rarely has access to the publications of foreign learned societies. This particular memoir, too, seems almost indispensable to every spectrum-observer; for it contains the beautiful map of the solar spectrum, and the corresponding tables, upon the construction of which the Professor has spent so much labour, and so much also that is more rare and precious. Just as the astronomer must have his charts and catalogues of stars in order that he may identify objects known to former observers, and interpolate his own discoveries, so the spectrum-observer must have this map of the spectrum at his elbow.

The portion of the spectrum represented in these maps extends from the line D to some distance beyond the line F, occupying a length of about 4 feet. The completion of the work between the lines A and G is delayed by the unhappy failure of the Professor's eyesight. The following small fragment, less than $\frac{1}{100}$ th part of the whole, will serve as a specimen of the maps; it represents the commencement at the remarkable double line D.

The Professor has also tabulated his results; and the following is the part of the Table corresponding to the figure.

Col. 1.	2.	3.	4.
1000·0	1	<i>a</i>	
1000·4	1	<i>a</i>	
1001·4	1	<i>a</i>	
1002·8	6	<i>b</i>	Na
1005·0	2	<i>b</i>	Ni
1006·8	6	<i>b</i>	Na
1011·2	3	<i>a</i>	



Here we have in col. 1 the distance of the line from the commencement of the scale, and, by difference, of course, from the adjoining lines; in col. 2 the darkness of each line according to the scale of numbers 1, 2, 3, 4, 5, 6; in col. 3 the breadth of each line according to the scale *a, b, c, d, e, f, g*; and lastly, in col. 4 the symbol of the element in whose spectrum is found a corresponding bright line,—the inference, as afterwards shown, being that this element exists in the sun's atmosphere. Prof. Kirchhoff proposes that each line should be referred to by its number or position on the arbitrary millimetre scale which he has adopted: thus the two lines of Fraunhofer's line D would be called 1002·8 and 1006·8; and Fraunhofer's *b* consists of 1633·4, 1648·3, and 1655·0. It is a great advantage of this proposed mode of reference, that it admits of the interpolation of the indefinite number of finer and almost nebulous lines which any

increase of the refracting power renders manifest. The method is obviously comparable to the determination of a star by its right ascension and declination. Not only, however, is the zero of Kirchhoff's scale placed quite arbitrarily, but he also states that "a relation between the numbers on the scale corresponding to the individual lines and the refractive indices of my prisms does not exist, because the prisms were sometimes placed more exactly than at other times at the angle of minimum deviation for the particular rays." The only natural mode of determining a line, as opposed to an arbitrary mode, is of course the refractive index in some medium of invariable composition, such as water, or bisulphide of carbon. But there are great difficulties in the way of determining refractive indices with sufficient exactness, and, provisionally at least, Kirchhoff's scale is certain to be adopted. The whole subject would be thrown into great confusion were different scales, each with its own zero, adopted by different observers, like the several systems of longitude; we earnestly deprecate such a proceeding.

We must pass rapidly over the second and third parts of the memoir, which treat briefly but pretty completely of "The Spectra of the Chemical Elements," and "The Reversal of the Spectra of Coloured Flames." They contain a review of results which the Professor had previously published in conjunction with Prof. Bunsen*; but the purely chemical and mathematical details are omitted and many facts added, especially concerning the spectra of the electric light. The short historical notices of researches in this subject given on pages 6 and 7 are imperfect; for they do not advert to Wollaston's discovery of the dark lines, and his examination of several kinds of light in 1802, nor to the still earlier observations of Thomas Melville. This latter experimentalist seems to have been the first discoverer in the branch of research with which we are occupied: some account of him will be found in the 'Chemical News' for May 3, 1862.

The solar spectrum, it is well known, is scored by dark lines, while the spectra of the various elements contain bright lines; and it was not unknown to previous observers (Fraunhofer, Brewster, Foucault, and perhaps others) that the bright yellow sodium line corresponded with Fraunhofer's dark line D. In the course of Kirchhoff and Bunsen's researches, it became manifest that a great number of the solar dark lines correspond to bright lines in the spectra of various metals; 149 such coincidences are indicated in the Map and Table which we have described. Hence it became very probable that the solar spectrum is the reverse or negative of a complicated positive spectrum, in which are combined the spectra of a great many elements. The solar spectrum is related to the spectra of the elements just as a collodiotype negative to its positive copies.

Reflecting on this remarkable fact, Kirchhoff was led to a further important discovery, developed in his *theory of exchanges*†. He has

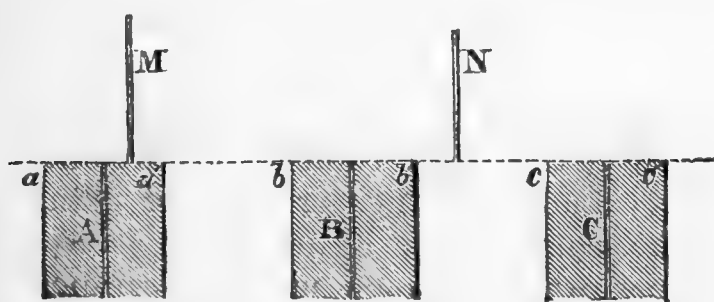
* See Phil. Mag. S. 4. vol. xx. pp. 1 and 89.

† Mr. Balfour Stewart, it is well known, had previously arrived at the same important conclusion by a separate path (Trans. Roy. Soc. of Edinb. vol. xxii. 1858). See also the paper of M. Ångström at the commencement of this Number of the Phil. Mag.

proved both experimentally and theoretically that a gas has the power of absorbing the very same rays of light which, when ignited, it gives off. The positive spectrum is from the rays of light produced by the gas; the negative spectrum is due to absorption of the same rays when white light from some extraneous source is passed through the gas. Kirchhoff hence arrives at the conclusion that the sun consists of an ignited solid or liquid central body, throwing off intense white light, and of an atmosphere containing many of our terrestrial elements in a state of vapour, yet capable of absorbing parts of the white light. The inductive proof of this interesting conclusion is one of the most beautiful problems which the philosophy of Bacon has presented; and we shall gladly dwell upon it.

Of the 676 lines registered in Kirchhoff's Map and Table, 141 are marked as corresponding with lines in the spectra of certain metals; in 8 cases there is coincidence with the lines of two metals; and of the coincidences, 60 occur with lines in the very complicated spectrum of iron. Taking iron as an instance, it has been proved that iron in the sun may produce the negative spectrum corresponding to that of iron, which we detect with surprise in the solar spectrum. Yet it may be that, of the 676 lines observed, 60 have by mere chance occurred in the precise positions in which iron would have produced them. Required the probability that the correspondence is due to chance, not to the presence of iron in the sun.

The 676 lines in the Professor's Map occur in a length of 1250 millims.; so that the average interval between each two lines may be taken at 2 millims. The method of observing coincidences was so far exact that, when a bright line of the iron spectrum came within $\frac{1}{2}$ millim. of a dark line of the solar spectrum, there seemed to be coincidence. We may express this in the adjoining figure by supposing the dark and, in reality, infinitely narrow lines A, B, C, &c. to be arranged at equal intervals of 2 millims., each being spread



out $\frac{1}{2}$ millim. on each side. Thus each dark line has an apparent width of 1 millim., and the lines altogether occupy half the length of the spectrum.

Now suppose an infinitely thin bright line of the iron spectrum to fall by pure chance upon such a spectrum. It is obviously just an equal chance that it will fall upon one of the dark spaces aa' , bb' , cc' , &c., as upon one of the bright spaces $a'b$, $b'c$, &c. If it fall like N upon a bright space, there will be no coincidence; if it fall, however, like M, ever so little within the boundary of the dark space, there will be at least apparent coincidence. Each of the 60 iron lines in

succession being thrown upon the solar spectrum, the probability of falling on a dark space, producing coincidence, is in each separate case *one-half*. The probability that all the 60 lines will fall in coincidence is found by multiplying together all the separate probabilities; it is then $\left(\frac{1}{2}\right)^{60}$, or less than $\frac{1}{1,152,930,000,000,000,000}$. In other words, the odds are more than 1,000,000,000,000,000,000 to 1 against all the 60 bright lines having fallen into coincidence with dark lines of the solar spectrum, had chance alone governed the distribution. We cannot but believe, then, that there was something more than chance in the matter.

Otherwise stated, there are more than a *trillion* of modes in which the 60 bright lines might be distributed among the dark lines, as regards coincidence and non-coincidence. Of these the very mode which occurs, that of perfect coincidence, is that which would occur were there iron in the sun's atmosphere. The conclusion is irresistible that there *is* iron in the sun's atmosphere. It is proved, as Kirchhoff says, "with as great a degree of certainty as we can attain in any question of physical science."

In this calculation we have indeed supposed the dark solar lines to be placed at equal intervals; but all the probabilities remain exactly the same, however irregular their distribution, provided the dark spaces *aa'*, *bb'*, *cc'*, &c. are equal to half the whole spectrum; that is to say, provided no two of the lines A, B, C, &c. approach within less than one millimetre of each other. As, however, many lines do approach nearer than this, the probability of mere casual coincidences is even less than we estimated. If we add the fact that the lines are observed to correspond not only in position, but also in relative brightness, the problem becomes vastly more complex, but the conclusion immensely more certain. Finally, as the prismatic apparatus is improved, the apparent breadth of the lines will be diminished, their number greatly increased. The coincidences will therefore become both greatly more numerous, and each coincidence will have a higher value in the elimination of chance; the proof attains an indefinite degree of certainty limited only by truth itself.

If the reader object that these coincidences are only apparent, and that we can never know whether or not the infinitely thin lines A, B, C, M, N, &c. coincide, he is not wrong, but possibly forgets that all inductive truths rest upon mere appearance, and involve a margin of error or mistake in observation. This problem of the iron lines in the solar spectrum we look upon as an epitome of the inductive method of science, identically the same, indeed, as any other inductive problem in principle, but beautifully simple and luminous in its procedure. And thus Kirchhoff's memoir seems to us not only a very interesting, but also a most improving subject of study.

The occurrence of iron in the sun's atmosphere having been thus rendered practically certain, the Professor proceeds to prove the undoubted presence or apparent absence of many other elements in the sun's atmosphere. Nothing can be more gratifying than the first acquisition of such a far-reaching mode of investigation and proof as

spectrum-observation. Like Galileo searching the heavens with his newly-made telescope, the Professor becomes for the time the wisest of men, and reveals the composition of the sun with a kind of dignity worthy of the occasion.

Before closing his memoir, Kirchhoff enters into a very interesting discussion as to the constitution of the sun's surface and atmosphere. After totally refuting the old physical hypothesis of Sir William Herschel, Arago, and others, he concludes that the sun's spots, instead of being cavities in a *photosphere*, are black clouds elevated above the luminous surface of the sun's body; he shows also that the perspective-phenomena which led Alexander Wilson to consider the spots as cavities, are at least partially explicable on this cloud theory. After much reflection, however, we are inclined to think that this new proposition is not only inconsistent with the general testimony and belief of those who have most minutely examined the sun's surface, but is decisively contradicted by the following observation of Sir William Herschel* :—

“Oct. 13, 1794. The spot in the sun I observed yesterday is drawn so near the margin, that the elevated side of the following part of it hides all the black ground, and still leaves the cavity visible, so that the depression of the black spots and the elevation of the faculæ are equally evident.”

We might point also to the original drawing by Wilson of the great spot of 1769† as partially exhibiting the same appearance. But the subject is one eminently debatable and worthy of debate: no one at all interested in the subject can omit studying Professor Kirchhoff's able arguments; but every reader is as yet, we think, free to draw his own conclusions.

XI. *Proceedings of Learned Societies.*

ROYAL INSTITUTION OF GREAT BRITAIN.

June 6, 1862.—The Duke of Northumberland, K.G., F.R.S.,
President, in the Chair.

“**O**N Force.” By John Tyndall, Esq., F.R.S., Professor of
Natural Philosophy, Royal Institution.

The existence of the International Exhibition suggested to our Honorary Secretary the idea of devoting the Friday evenings after Easter of the present year to discourses on the various agencies on which the material strength of England is based. He wished to make iron, coal, cotton, and kindred matters the subjects of these discourses,—opening the series by a discourse on the Great Exhibition itself. And he wished me to finish the series by a discourse on “Force” in general. For some months I thought over the subject at intervals, and had devised a plan of dealing with it; but three weeks ago I was induced to swerve from this plan, for reasons which shall be made known towards the conclusion of the discourse.

* Phil. Trans. 1795, p. 58.

† Ibid. 1774, p. 8.

We all have ideas more or less distinct regarding force ; we know in a general way what muscular force means, and each of us would less willingly accept a blow from a pugilist than have his ears boxed by a lady. But these general ideas are not now sufficient for us ; we must learn how to express numerically the exact mechanical value of the two blows : this is the first point to be cleared up.

[A sphere of lead weighing one pound was suspended at a height of 16 feet above the theatre floor. It was liberated, and fell by gravity.] That weight required exactly a second to fall to the earth from that elevation ; and the instant before it touched the earth, it had a velocity of 32 feet a second. That is to say, if at that instant the earth were annihilated, and its attraction annulled, the weight would proceed through space at the uniform velocity of 32 feet a second.

Suppose that, instead of being pulled downward by gravity, the weight is cast upward in opposition to the force of gravity, with what velocity must it start from the earth's surface in order to reach a height of 16 feet ? With a velocity of 32 feet a second. This velocity imparted to the weight by the human arm, or by any other mechanical means, would carry the weight up to the precise height from which it has fallen.

Now the lifting of the weight may be regarded as so much mechanical work. I might place a ladder against the wall, and carry the weight up a height of 16 feet ; or I might draw it up to this height by means of a string and pulley, or I might suddenly jerk it up to a height of 16 feet. The amount of work done in all these cases, as far as the raising the weight is concerned, would be absolutely the same. The absolute amount of work done depends solely upon two things : first of all, on the quantity of matter that is lifted ; and secondly, on the height to which it is lifted. If you call the quantity or mass of matter m , and the height through which it is lifted h , then the product of m into h , or mh , expresses the amount of work done.

Supposing, now, that, instead of imparting a velocity of 32 feet a second to the weight, we impart twice this speed, or 64 feet a second. To what height will the weight rise ? You might be disposed to answer, " 'To twice the height ;" but this would be quite incorrect. Both theory and experiment inform us that the weight would rise to four times the height : instead of twice 16, or 32 feet, it would reach four times 16, or 64 feet. So also, if we treble the starting velocity, the weight would reach nine times the height ; if we quadruple the speed at starting, it would attain sixteen times the height. Thus, with a velocity of 128 feet a second at starting, the weight would attain an elevation of 256 feet. Supposing we augment the velocity of starting seven times, we should raise the weight to 49 times the height, or to an elevation of 784 feet.

Now the work done, or, as it is sometimes called, the *mechanical effect*, as before explained, is proportional to the height ; and as a double velocity gives four times the height, a treble velocity nine times the height, and so on, it is perfectly plain that the mechanical effect increases as the square of the velocity. If the mass of

the body be represented by the letter m , and its velocity by v , then the mechanical effect would be represented by mv^2 . In the case considered, I have supposed the weight to be cast upward, being opposed in its upward flight by the resistance of gravity; but the same holds true if I send the projectile into water, mud, earth, timber, or other resisting material. If, for example, you double the velocity of a cannon-ball, you quadruple its mechanical effect. Hence the importance of augmenting the velocity of a projectile, and hence the philosophy of Sir William Armstrong in using a 50 lb. charge of powder in his recent striking experiments.

The measure, then, of mechanical effect is the mass of the body multiplied by the square of its velocity.

Now in firing a ball against a target, the projectile, after collision, is often found hissing hot. Mr. Fairbairn informs me that in the experiments at Shoeburyness it is a common thing to see a flash of light, even in broad day, when the ball strikes the target. And if I examine my lead weight after it has fallen from a height I also find it heated. Now here experiment and reasoning lead us to the remarkable law that the amount of heat generated, like the mechanical effect, is proportional to the product of the mass into the square of the velocity. Double your mass, other things being equal, and you double your amount of heat; double your velocity, other things remaining equal, and you quadruple your amount of heat. Here, then, we have common mechanical motion destroyed and heat produced. I take this violin-bow and draw it across this string. You hear the sound. That sound is due to motion imparted to the air, and to produce that motion a certain portion of the muscular force of my arm must be expended. We may here correctly say that the mechanical force of my arm is converted into music. And in a similar way we say that the impeded motion of our descending weight, or of the arrested cannon-ball, is converted into heat. The mode of motion changes, but it still continues motion; *the motion of the mass is converted into a motion of the atoms of the mass*; and these small motions, communicated to the nerves, produce the sensation which we call heat. We moreover know the amount of heat which a given amount of mechanical force can develop. Our lead ball, for example, in falling to the earth, generated a quantity of heat sufficient to raise the temperature of its own mass three-fifths of a Fahrenheit degree. It reached the earth with a velocity of 32 feet a second; and forty times this velocity would be a small one for a rifle-bullet: multiplying $\frac{3}{5}$ ths by the square of 40, we find that the amount of heat developed by collision with the target would, if wholly concentrated in the lead, raise its temperature 960 degrees. This would be more than sufficient to fuse the lead. In reality, however, the heat developed is divided between the lead and the body against which it strikes; nevertheless it would be worth while to pay attention to this point, and to ascertain whether rifle-bullets do not, under some circumstances, show signs of fusion.

From the motion of sensible masses by gravity and other means, the speaker passed to the motion of atoms towards each other by

chemical affinity. A collodion balloon filled with a mixture of chlorine and hydrogen was hung in the focus of a parabolic mirror, and in the focus of a second mirror 20 feet distant a strong electric light was suddenly generated; the instant the light fell upon the balloon, the atoms within it fell together with explosion, and hydrochloric acid was the result. The burning of charcoal in oxygen was an old experiment, but it had now a significance beyond what it used to have; we now regard the act of combination on the part of the atoms of oxygen and coal exactly as we regard the clashing of a falling weight against the earth. And the heat produced in both cases is referable to a common cause. This glowing diamond, which burns in oxygen as a star of white light, glows and burns in consequence of the falling of the atoms of oxygen against it. And could we measure the velocity of the atoms when they clash, and could we find their number and weight, multiplying the mass of each atom by the square of its velocity, and adding all together, we should get a number representing the exact amount of heat developed by the union of the oxygen and carbon.

Thus far we have regarded the heat developed by the clashing of sensible masses and of atoms. Work is expended in giving motion to these atoms or masses, and heat is developed. But we reverse this process daily, and by the expenditure of heat execute work. We can raise a weight by heat; and in this agent we possess an enormous store of mechanical power. This pound of coal, which I hold in my hand, produces by its combination with oxygen an amount of heat which, if mechanically applied, would suffice to raise a weight of 100 lbs. to a height of 20 miles above the earth's surface. Conversely, 100 lbs. falling from a height of 20 miles, and striking against the earth, would generate an amount of heat equal to that developed by the combustion of a pound of coal. Wherever work is done by heat, heat disappears. A gun which fires a ball is less heated than one which fires blank cartridge. The quantity of heat communicated to the boiler of a working steam-engine is greater than that which could be obtained from the recondensation of the steam after it had done its work; and the amount of work performed is the exact equivalent of the amount of heat lost. Mr. Smyth informed us in his interesting discourse, that we dig annually 84 millions of tons of coal from our pits. The amount of mechanical force represented by this quantity of coal seems perfectly fabulous. The combustion of a single pound of coal, supposing it to take place in a minute, would be equivalent to the work of 300 horses; and if we suppose 108 millions of horses working day and night with unimpaired strength, for a year, their united energies would enable them to perform an amount of work just equivalent to that which the annual produce of our coal-fields would be able to accomplish.

Comparing with ordinary gravity the energy of the force with which oxygen and carbon unite together, the chemical affinity seems almost infinite. But let us give gravity fair play; let us permit it to act throughout its entire range. Place a body at such a distance

from the earth that the attraction of the earth is barely sensible, and let it fall to the earth from this distance. It would reach the earth with a final velocity of 36,747 feet in a second; and on collision with the earth the body would generate about twice the amount of heat generated by the combustion of an equal weight of coal. We have stated that by falling through a space of 16 feet our lead bullet would be heated three-fifths of a degree; but a body falling from an infinite distance has already used up 1,299,999 parts out of 1,300,000 of the earth's pulling power, when it has arrived within 16 feet of the surface; on this space only $\frac{1}{1300000}$ th of the whole force is exerted.

Let us now turn our thoughts for a moment from the earth towards the sun. The researches of Sir John Herschel and M. Pouillet have informed us of the annual expenditure of the sun as regards heat; and by an easy calculation we ascertain the precise amount of the expenditure which falls to the share of our planet. Out of 2300 million parts of light and heat the earth receives one. The whole heat emitted by the sun in a minute would be competent to boil 12,000 millions of cubic miles of ice-cold water. How is this enormous loss made good? Whence is the sun's heat derived, and by what means is it maintained? No combustion, no chemical affinity with which we are acquainted would be competent to produce the temperature of the sun's surface. Besides, were the sun a burning body merely, its light and heat would assuredly speedily come to an end. Supposing it to be a solid globe of coal, its combustion would only cover 4600 years of expenditure. In this short time it would burn itself out. What agency then can produce the temperature and maintain the outlay? We have already regarded the case of a body falling from a great distance towards the earth, and found that the heat generated by its collision would be twice that produced by the combustion of an equal weight of coal. How much greater must be the heat developed by a body falling towards the sun! The maximum velocity with which a body can strike the earth is about 7 miles in a second; the maximum velocity with which it can strike the sun is 390 miles in a second. And as the heat developed by the collision is proportional to the square of the velocity destroyed, an asteroid falling into the sun with the above velocity would generate about 10,000 times the quantity of heat generated by the combustion of an asteroid of coal of the same weight. Have we any reason to believe that such bodies exist in space, and that they may be raining down upon the sun? The meteorites flashing through the air are small planetary bodies, drawn by the earth's attraction, and entering our atmosphere with planetary velocity. By friction against the air they are raised to incandescence and caused to emit light and heat. At certain seasons of the year they shower down upon us in great numbers. In Boston 240,000 of them were observed in nine hours. There is no reason to suppose that the planetary system is limited to "vast masses of enormous weight;" there is every reason to believe that space is stocked with smaller masses, which obey the same laws as the large

ones. That lenticular envelope which surrounds the sun, and which is known to astronomers as the zodiacal light, is probably a crowd of meteors; and moving as they do in a resisting medium they must continually approach the sun. Falling into it, they would be competent to produce the heat observed, and this would constitute a source from which the annual loss of heat would be made good. The sun, according to this hypothesis, would be continually growing larger; but how much larger? Were our moon to fall into the sun it would develop an amount of heat sufficient to cover one or two years' loss; and were our earth to fall into the sun a century's loss would be made good. Still, our moon and our earth, if distributed over the surface of the sun, would utterly vanish from perception. Indeed, the quantity of matter competent to produce the necessary effect would, during the range of history, produce no appreciable augmentation in the sun's magnitude. The augmentation of the sun's attractive force would be more appreciable. However this hypothesis may fare as a representant of what is going on in nature, it certainly shows how a sun might be formed and maintained by the application of known thermo-dynamic principles.

Our earth moves in its orbit with a velocity of 68,040 miles an hour. Were this motion stopped, an amount of heat would be developed sufficient to raise the temperature of a globe of lead of the same size as the earth 384,000 degrees of the Centigrade thermometer. It has been prophesied that "the elements shall melt with fervent heat." The earth's own motion embraces the conditions of fulfilment; stop that motion, and the greater part, if not the whole, of her mass would be reduced to vapour. If the earth fell into the sun, the amount of heat developed by the shock would be equal to that developed by the combustion of 6435 earths of solid coal.

There is one other consideration connected with the permanence of our present terrestrial conditions, which is well worthy of our attention. Standing upon one of the London bridges, we observe the current of the Thames reversed, and the water poured upward twice a day. The water thus moved rubs against the river's bed and sides; and heat is the consequence of this friction. The heat thus generated is in part radiated into space, and then lost, as far as the earth is concerned. What is it that supplies this incessant loss? The earth's rotation. Let us look a little more closely at the matter. Imagine the moon fixed, and the earth turning like a wheel from west to east in its diurnal rotation. Suppose a high mountain on the earth's surface; on approaching the moon's meridian that mountain is, as it were, laid hold of by the moon, and forms a kind of handle by which the earth is pulled more quickly round. But when the meridian is passed the pull of the moon on the mountain would be in the opposite direction, it now tends to diminish the velocity of rotation as much as it previously augmented it; and thus the action of all fixed bodies on the earth's surface is neutralized. But suppose the mountain to lie *always* to the east of the moon's meridian, the pull then would be always exerted against the earth's rotation, the velocity of which would be diminished in a degree corresponding

to the strength of the pull. *The tidal wave occupies this position* : it lies always to the east of the moon's meridian ; and thus the waters of the ocean are in part dragged as a brake along the surface of the earth, and as a brake they must diminish the velocity of the earth's rotation. The diminution, though inevitable, is, however, too small to make itself felt within the period over which observations on the subject extend. Supposing then that we turn a mill by the action of the tide, and produce heat by the friction of the millstones ; that heat has an origin totally different from the heat produced by another mill which is turned by a mountain stream. The former is produced at the expense of the earth's rotation, the latter at the expense of the sun's radiation.

The sun, by the act of vaporization, lifts mechanically all the moisture of our air. It condenses and falls in the form of rain ; it freezes and falls as snow. In this solid form it is piled upon the Alpine heights, and furnishes materials for the glaciers of the Alps. But the sun again interposes, liberates the solidified liquid, and permits it to roll by gravity to the sea. The mechanical force of every river in the world, as it rolls towards the ocean, is drawn from the heat of the sun. No streamlet glides to a lower level without having been first lifted to the elevation from which it springs by the mighty power of the sun. The energy of winds is also due entirely to the sun ; but there is still another work which he performs, and his connexion with which is not so obvious. Trees and vegetables grow upon the earth, and when burned they give rise to heat, and hence to mechanical energy. Whence is this power derived ? You see this oxide of iron, produced by the falling together of the atoms of iron and oxygen ; here also is a transparent gas which you cannot now see (carbonic acid gas) which is formed by the falling together of carbon and oxygen. These atoms thus in close union resemble our lead weight while resting on the earth : but I can wind up the weight and prepare it for another fall ; and so these atoms can be wound up, separated from each other, and thus enabled to repeat the process of combination. In the building of plants carbonic acid is the material from which the carbon of the plant is derived ; and the solar beam is the agent which tears the atoms asunder, setting the oxygen free, and allowing the carbon to aggregate in woody fibre. Let the solar rays fall upon a surface of sand : the sand is heated, and finally radiates away as much heat as it receives : let the same beams fall upon a forest ; the quantity of heat given back is less than the forest receives, for the energy of a portion of the sunbeams is invested in building up the trees in the manner indicated. Without the sun the reduction of the carbonic acid cannot be effected ; and an amount of sunlight is consumed exactly equivalent to the molecular work done. Thus trees are formed ; thus the cotton on which Mr. Bazley discoursed last Friday is formed. I ignite this cotton, and it flames ; the oxygen again unites with its beloved carbon ; but an amount of heat equal to that which you see produced by its combustion was sacrificed by the sun to form that bit of cotton.

But we cannot stop at vegetable life ; for this is the source, mediate or immediate, of all animal life. The sun severs the carbon from its oxygen ; the animal consumes the vegetable thus formed, and in its arteries a reunion of the severed elements takes place, and produces animal heat. Thus, strictly speaking, the process of building a vegetable is one of winding up ; the process of building an animal is one of running down. The warmth of our bodies, and every mechanical energy which we exert, trace their lineage directly to the sun. The fight of a pair of pugilists, the motion of an army, or the lifting of his own body up mountain-slopes by an Alpine climber, are all cases of mechanical energy drawn from the sun. Not, therefore, in a poetical, but in a purely mechanical sense, are we children of the sun. Without food we should soon oxidize our own bodies. A man weighing 150 lbs. has 64 lbs. of muscle ; but these, when dried, reduce themselves to 15 lbs. Doing an ordinary day's work, for eighty days, this mass of muscle would be wholly oxidized. Special organs which do more work would be more quickly oxidized : the heart, for example, if entirely unsustained, would be oxidized in about a week. Take the amount of heat due to the direct oxidation of a given amount of food ; a less amount of heat is developed by this food in the working animal frame, and the missing quantity is the exact equivalent of the mechanical work which the body accomplishes.

I might extend these considerations ; the work, indeed, is done to my hand ; but I am warned that I have kept you already too long. To whom, then, are we indebted for the striking generalizations of this evening's discourse ? All that I have laid before you is the work of a man of whom you have scarcely ever heard. All that I have brought before you has been taken from the labours of a German physician, named Mayer. Without external stimulus, and pursuing his profession as town physician in Heilbronn, this man was the first to raise the conception of the interaction of natural forces to clearness in his own mind. And yet he is scarcely ever heard of in scientific lectures, and even to scientific men his merits are but partially known. Led by his own beautiful researches, and quite independent of Mayer, Mr. Joule published his first Paper " On the Mechanical Value of Heat " in 1843 ; but in 1842 Mayer had actually calculated the mechanical equivalent of heat from data which a man of rare originality alone could turn to account. From the velocity of sound in air, Mayer determined the mechanical equivalent of heat. In 1845 he published his Memoir on " Organic Motion," and applied the mechanical theory of heat in the most fearless and precise manner to vital processes. He also embraced the other natural agents in his chain of conservation. In 1853 Mr. Waterston proposed, independently, the meteoric theory of the sun's heat ; and in 1854 Professor William Thomson applied his admirable mathematical powers to the development of the theory ; but six years previously the subject had been handled in a masterly manner by Mayer, and all that I have said on the subject has been derived from him. When we consider the circumstances of Mayer's life, and the

period at which he wrote, we cannot fail to be struck with astonishment at what he has accomplished. Here was a man of genius working in silence, animated solely by a love of his subject, and arriving at the most important results some time in advance of those whose lives were entirely devoted to Natural Philosophy. It was the accident of bleeding a feverish patient at Java in 1840 that led Mayer to speculate on these subjects. He noticed that the venous blood in the tropics was of a much brighter red than in colder latitudes; and his reasoning on this fact led him into the laboratory of natural forces, where he has worked with such signal ability and success. Well, you will desire to know what has become of this man. His mind gave way; he became insane, and he was sent to a lunatic asylum. In a biographical dictionary of his country it is stated that he died there: but this is incorrect. He recovered, and, I believe, is at this moment a cultivator of vineyards in Heilbronn.

While preparing for publication my last course of lectures on Heat, I wished to make myself acquainted with all that Mayer had done in connexion with this subject. I accordingly wrote to two gentlemen who above all others seemed likely to give me the information which I needed. Both of them are Germans, and both particularly distinguished in connexion with the Dynamical Theory of Heat. Each of them kindly furnished me with the list of Mayer's publications; and one of them was so friendly as to order them from a bookseller, and to send them to me. This friend, in his reply to my first letter regarding Mayer, stated his belief that I should not find anything very important in Mayer's writings; but before forwarding the memoirs to me, he read them himself. His letter accompanying the first of these papers contains the following words:—"I must here retract the statement in my last letter, that you would not find much matter of importance in Mayer's writings: I am astonished at the multitude of beautiful and correct thoughts which they contain;" and he goes on to point out various important subjects, in the treatment of which Mayer had anticipated other eminent writers. My second friend, in whose own publications the name of Mayer repeatedly occurs, and whose papers containing these references were translated some years ago by myself, was, on the 10th of last month, unacquainted with the thoughtful and beautiful essay of Mayer's entitled *Beiträge zur Dynamik des Himmels*; and in 1854, when Professor William Thomson developed in so striking a manner the meteoric theory of the sun's heat, he was certainly not aware of the existence of that essay, though, from a recent article in 'Macmillan's Magazine,' I infer that he is now aware of it. Mayer's physiological writings have been referred to by physiologists (by Dr. Carpenter, for example) in terms of honourable recognition. We have hitherto, indeed, obtained fragmentary glimpses of the man, partly from physicists, and partly from physiologists; but his total merit has never yet been recognized as it assuredly would have been had he chosen a happier mode of publication. I do not think a greater disservice could be done to a man

of science than to overstate his claims: such overstatement is sure to recoil to the disadvantage of him in whose interest it is made. But when Mayer's opportunities, achievements, and fate are taken into account, I do not think that I shall be deeply blamed for attempting to place him in that honourable position which I believe to be his due.

Here, however, are the titles of Mayer's papers, the perusal of which will correct any error of judgment into which I may have fallen regarding their author. "*Bemerkungen über die Kräfte der umbelebten Natur*," Liebig's *Annalen*, 1842, vol. xlii. p. 231; *Die organische Bewegung in ihrem Zusammenhange mit dem Stoffwechsel*, Heilbronn, 1845; *Beiträge zur Dynamik des Himmels*, Heilbronn, 1848; *Bemerkungen über das mechanische Equivalent der Wärme*, Heilbronn, 1851.

ROYAL SOCIETY.

[Continued from vol. xxiii. p. 557.]

June 20, 1861.—Major-General Sabine, R.A., Treasurer and Vice-President, in the Chair.

The following communications were read:—

"On the Lunar Semidiurnal Variation of the Barometer." By John Allan Broun, Esq., F.R.S.

The results obtained hitherto for the lunar diurnal variation of atmospheric pressure have been received with some doubt; the range of the variation has been found to be small, and the proximity to the sea of the stations for which the discussions have been made, has given some value to the idea that the variation is due simply to the tidal rise and fall of the sea, the base of the atmosphere. It has even been suggested that the barometer should be incapable of showing an atmospheric tide, because the gravity of the mercury is diminished by the lunar attraction, as well as that of the atmosphere; this suggestion, however, omits all consideration of the integrating means by which a tide is formed.

I have considered the question at two stations in the following manner. Simultaneous observations of two standard barometers of the same construction, with boiled tubes 0·65 inch internal diameter, were made hourly during fifteen months (April 1857 to June 1858) at two observatories; one at Trevandrum 200 feet above the sea-level and three miles distant from the sea, the other on the summit of the Agustier peak of the South Indian Ghats, 6200 feet above the sea, about 22 miles from Trevandrum, 25 miles from the sea on the west, and 40 to 60 miles from the sea on the south and east. The means of all the observations give the following results.

At Trevandrum.—The lunar diurnal variation of atmospheric pressure has two nearly equal maxima, occurring almost exactly at the moon's passages of the upper and lower meridian, the minima occurring six hours before and after these epochs.

At Agustier (6200 feet).—The law is nearly the same as to epochs, the maximum for the inferior passage occurring, however, about one hour later, and each minimum occurring about one hour nearer the upper passage.

The chief difference in the two results is that between the relative values of the maxima and minima. The oscillation occurring while the moon is below the horizon, or between the meridians of 6^h and of 18^h, has the same amount at Trevandrum (height 200 feet) and at Agustier (height 6200 feet); but the oscillation occurring while the moon is above the horizon, has twice the amount at the lower station which it has at the upper station.

This curious result, which it is difficult to explain by any theory depending simply on the attraction of gravitation, whether acting on the sea or the atmosphere, agrees remarkably with the conclusions for the solar diurnal oscillation obtained from the discussion of hourly observations made simultaneously during a month at five different stations, four of these being on the Agustier mountains, rising gradually from 1200 to 6200 feet, the fifth station being at Trevandrum. From these observations (an abstract of which will be found noticed in the Report of the British Association for 1859, Trans. of Sect., p. 46), it appeared that the solar semidiurnal oscillation between 9 P.M. and 9 A.M. was nearly the same at all the stations; while the day oscillation, 9 A.M. to 9 P.M., diminished with the height, being at 6200 feet little more than half the amount of the oscillation at 200 feet.

These facts might be put into the following general form:—When the heavenly body (sun or moon) is *below the horizon*, the semidiurnal oscillation of the barometer within the tropics has the same value at all heights up to 6200 feet; but when the body is *above the horizon*, the oscillation at 6200 feet has only half the amount of the oscillation at the level of the sea.

“On the Law of Disturbance and the Range of the Diurnal Variation of Magnetic Declination near the Magnetic Equator, with reference to the Moon’s Hour-angle.” By John Allan Broun, Esq., F.R.S.

The discovery by Dr. Lamont of a “decennial” period in the range of the solar diurnal variation of magnetic declination, naturally leads to the question whether a similar law may not exist for the lunar diurnal variation; the question is also of importance in connexion with the theory of the cause of these variations. The following results for the range of the lunar diurnal variation were obtained from the discussion whose conclusions were given in the Proceedings of the Royal Society, vol. x. p. 475.

Range of Diurnal Variation at Trevandrum, 1854–59.

Year.	Lunar.		Solar.		Ratio. Solar <hr/> Lunar.
	From variations for groups of 3 months.		From variations for single months.		
1854	0·52	2·24	4·3
1855	0·54	2·05	3·6
1856	0·41	2·01	4·9
1857	0·53	2·15	4·0
1858	0·58	2·41	4·2
1859	0·66	2·64	4·0

It appears from these values that the range of the lunar diurnal period varies from year to year according to the same law (nearly) as the range of the solar diurnal period ; and we may conclude that the range of lunar diurnal variation obeys the “decennial” law.

It is well known that the declination needle obeys two diurnal laws due to the solar action ; the first of these governs the movement of the north end of the needle from east to west and from west to east, the second determines a superposed movement resulting from the relative amounts of displacement of the needle from its normal position at each hour ; the latter is usually termed the diurnal law of disturbance. In order to obtain this law and the values of the disturbance, the quantities $d_0, d_1, d_2 \dots$ (Proceedings, vol. x. p. 477) are summed for each hour and the means are taken. No similar investigation, as far as I am aware, has as yet been undertaken for the lunar diurnal variation.

I have now arrived at the following conclusions in the discussion for this object of six years' observations of magnetic declination made at the Trevandrum Observatory.

Having arranged the differences containing the lunar effect,

$$\begin{array}{ccccccc} d'_0 & d'_1 & d'_2 & \dots & \dots & \dots & \dots \\ d''_0 & d''_1 & d''_2 & \dots & \dots & \dots & \dots \\ \vdots & \vdots & \vdots & & & & \\ \vdots & \vdots & \vdots & & & & \end{array}$$

according to the moon's hour-angles from the meridian, and obtained the means,

$$\frac{\Sigma d'_0}{n-1} = h + \frac{\Sigma_{23}^0(x')}{n-1}, \quad \frac{\Sigma d'_1}{n-1} = h_1 + \frac{\Sigma_{23}^0(x'')}{n-1} \dots \dots,$$

the second differences

$$\begin{array}{ccccccc} d'_0 - (h_0), & d'_1 - (h_1) & \dots & \dots & \dots & \dots & \dots \\ d''_0 - (h_0), & d''_1 - (h_1) & \dots & \dots & \dots & \dots & \dots \\ \vdots & \vdots & & & & & \\ \vdots & \vdots & & & & & \end{array}$$

were taken ; these differences, as in the case of the similar investigation for the solar laws, represent the disturbance at each hour-angle of the moon ; and when the means of the second differences are taken, the law of the lunar diurnal disturbance will be obtained, if any such law exist.

The small range of the regular lunar diurnal variation, and the derangement produced by the larger disturbances, the effects of which cannot be eliminated in short series of observations, would render identical results from different groups of years improbable, especially if the range of the variation to be determined be small. The following conclusions, however, seem sufficiently confirmed by the discussion of different groups of observations to be accepted.

There is a lunar diurnal law of disturbance which, from the mean of six years' observation, consists of a

Principal maximum 5 hours before the moon passes the upper meridian.

Principal minimum near the moon's passage of the lower meridian.

Secondary maximum 6 hours after the passage of the upper meridian.

Secondary minimum 1 hour after the passage of the upper meridian.

When the results for each year are compared, the principal maximum and minimum are always shown distinctly; but the values and epochs of the secondary maximum and minimum vary, and that so irregularly, that in the mean for the three years 1857-59 the existence of either is doubtful, only the principal maximum and minimum being well-marked.

In order to determine if the law varied with season, means of groups for quarters of the year were obtained; means also for the groups of half-years, October to March and April to September, were taken. As the results for the quarters forming each half-year agreed with each other, and that for the half-year which contained them, the following conclusions may be accepted as probably true.

Half-year October to March.

Principal maximum, $5\frac{1}{2}$ hours before the moon's passage of the upper meridian.

Principal minimum, 3 hours before the moon's passage of the lower meridian.

Secondary maximum, $5\frac{1}{2}$ hours after the moon's passage of the upper meridian.

Secondary minimum, 1 hour after the moon's passage of the upper meridian.

Half-year April to September.

Maximum, $4\frac{1}{4}$ hours before the moon's passage of the upper meridian.

Minimum, at the passage of the lower meridian.

In the case of the half-year including the June solstice, the secondary points disappear; this also is the case for each of the quarters forming the half-year. It may be due to this and the preponderance of the disturbance in one half of any given year, that the mean for the whole year shows distinctly or indistinctly the secondary points. the question, however, of the secondary points will be considered here after in another discussion including a longer series of observations.

The mean value of the lunar disturbance of magnetic }
declination for the six years 1854-59 at Trevandrum .. } = 0'.341

The maximum value at 5 hours before the upper passage = 0'.363

The minimum value near the inferior passage = 0'.324

So that the variation of the mean law is only 0'.04, or about two seconds and a half.

When we examine the means for the separate years, we find

<i>Mean Lunar Disturbance.</i>				Ratio of range of diurnal varia- tion to 1856.	
			Ratio to 1856.		
1854	=0.320	1.18 1.27
1855	0.295	1.08 1.31
1856	0.272	1.00 1.00
1857	0.326	1.20 1.28
1858	0.379	1.39 1.41
1859	0.453	1.67 1.59

I have not at hand the mean solar disturbances for each year, and cannot therefore compare the two; it will appear, however, from a comparison with the ranges of the solar diurnal variation (which are known to follow nearly the same law as the law of mean disturbance), that the mean lunar disturbance obeys the same law as the mean solar disturbance, and that the former is probably dependent upon the latter.

“Experimental Researches on the Functions of the Vagus and the Cervical Sympathetic Nerves in Man.” By Augustus Waller, M.D., F.R.S.

“On the Double Tangents of a Curve of the Fourth Order.” By Arthur Cayley, Esq., F.R.S.

“Notes on the Atmospheric Lines of the Solar Spectrum and on certain Spectra of Gases.” By Dr. John Hall Gladstone, F.R.S.

In the paper of Sir David Brewster and myself on the lines of the solar spectrum *, attention was drawn to the following among other phenomena:—

1st. “When the sun descends towards the horizon and shines through a rapidly increasing depth of air, certain lines which before were little if at all visible, became black and well defined, and dark bands appear even in what were formerly the most luminous parts of spectrum.” These we termed “atmospheric lines.” We did not wish to express by that term anything beyond the fact above mentioned; yet we threw out the idea that these lines may have their origin “in the air that encircles our globe.”

2nd. In the case of those artificial flames whose spectra “consist of a series of luminous bands separated by dark spaces . . . these luminous bands sometimes coincide with the dark lines of the solar spectrum.”

About the same time Kirchhoff † published his theory that this remarkable coincidence is due to the presence in the atmosphere of the sun of the substances which emit these luminous bands, and their appearance as *dark* lines is because “the spectrum of an incandescent gas becomes reversed, when a source of light of sufficient intensity, giving a continuous spectrum, is placed behind the luminous gas.” It also appears that “sodium vapour at a temperature much below that at which it becomes luminous, exerts its

* Philosophical Transactions, 1860, p. 149.

† Pogg. Ann. cix. pp. 148, 275; cx. p. 187.

absorptive power at exactly the same point of the spectrum as it does at the highest temperatures which we can produce." It is, however, as yet an undetermined question at what temperature below that at which it becomes luminous, a gas ceases to absorb; though, from the close connexion which has been shown to exist between emission and absorption, there can be little doubt that such a limit would be arrived at.

This theory of the origin of Fraunhofer's lines, and the uncertainty just alluded to, suggested the desirableness of comparing the "atmospheric lines" of absorption with the luminous bands which make their appearance in the linear spectra of the light emitted by the different constituents of the atmosphere when sufficiently heated. If all the rays emitted by these different constituents should be found to coincide with the "atmospheric lines," it would show at once the origin of the lines, and prove that gases at the ordinary temperature will absorb rays of the same refrangibility as those they emit when themselves glowing. If, on the contrary, they should be found not to coincide, it would prove by an extreme case (for we operate thus upon many miles of oxygen and nitrogen gases at least) that this connexion between absorption and emission either does not extend to these elements, or is confined within those narrower limits of temperature which theory seems to require. It was also conceivable that the atmospheric lines might coincide with the rays emitted, not by all, but by a portion of the constituents of the atmosphere.

The following data exist for this comparison. Ångström* has delineated the luminous bands due to the gas when the electric spark is sent through nitrogen, oxygen, carbonic acid, hydrogen, &c. Plücker† has described and measured those bands which appear in the spectra of the same and other gases when they serve as the residuary gas in Geissler's tubes. Under the influence of the electric discharge, the vapour of water, carbonic acid, ammonia, and other compounds are decomposed; but it occurred to me that the oxyhydrogen flame must consist partly, if not entirely, of glowing steam, since it is only when the two elements combine that the heat is evolved; and that similarly the flame of carbonic oxide burning in air must contain the light emitted from glowing carbonic acid. I therefore subjected these two flames to prismatic examination.

The result of the comparison has been that not one of these observations shows any accordance between the luminous bands due to the gas, and the dark lines that make their appearance in the solar spectrum when the sun is shining through a great depth of air. Hydrogen alone is inconclusive. Neither is there any accordance between these luminous bands and the more prominent lines of the ordinary solar spectrum.

This shows that oxygen and nitrogen, and perhaps other gases, though in enormous quantity, do not absorb at the ordinary temperature rays of the same refrangibility as those they emit when heated by the means specified.

It would not be legitimate to infer from this that the atmospheric lines have not their origin in the absorbent power of one or more of

* Pogg. *Ann.* xciv. p. 141.

† Ibid. cvii. pp. 497, 638.

the common constituents of the atmosphere. It is well known that some gases when placed before a continuous spectrum produce lines of absorption; among these are bromine and iodine vapour; yet the dark lines caused by these two halogens* do not coincide with the bright lines into which Plücker found the light of Geissler's tubes containing bromine and iodine to be resolved by the prism; nor have I succeeded in reversing them by bringing these substances into a very hot but little luminous flame. Bright lines were discerned, but in other positions. In connexion with this subject, it may be worth noting that a prismatic examination of the sun's rays passing through three inches of mercury vapour at above 300°C ., did not afford the least indication of the reversal of the bright rays that appear when that metal is rendered incandescent.

From the fact that the atmospheric lines do not always present the same appearance when the sun is on the horizon, and that the band δ has been observed during a shower, and the most prominent lines during a fog, it has been sometimes supposed that the aqueous vapour in the atmosphere is the cause of them. Yet this can scarcely be. They seem not to be due to little vesicles of condensed vapour; for the sun's rays when passing through the edges of a cloud do not exhibit them, unless, of course, near the horizon. And they seem not to be due to gaseous water; for they appear near sunset when the aqueous vapour in the atmosphere is reduced to a minimum by frosty weather, though they are not seen when the sun is higher up in the heavens on a damp warm day. From Sir David Brewster's notes, it appears that February 10, 11, 12, and 13 of the year 1838 were frosty days; yet the lines were well seen: δ , ι , η , C 6, C 15, C 16, and D are specially mentioned. On the 13th, when the thermometer stood at 23°F ., ι is said to have been the most prominent.

In the paper referred to at the commencement, we, in common with most of those who worked on the subject before the appearance of Kirchhoff and Bunsen's paper†, ascribed a bright line coincident with D to other spectra than that of soda. This was no doubt owing to the almost universal trace of that substance.

The electric lights produced between charcoal points by Professor Holmes's magneto-apparatus, and by a galvanic battery with M. Serin's lamp, were found to be identical, when subjected to prismatic analysis. Each exhibited a continuous spectrum, and not those variations of bright and dark which other observers, as well as myself, have noticed in electric lights from charcoal points of but inferior quality. The spectrum extended at both ends beyond that of ordinary direct sunlight, and the only lines which I could discern were bright ones in the violet or lavender region. The following refractive indices were determined for the magneto-electric light; and they are compared with the refractive indices of the nearest principal dark lines of the solar spectrum as determined with the same prism. The

* See "Experiments and Observations on some cases of lines in the prismatic spectrum produced by the passage of light through coloured vapours and gases, and from certain coloured flames," by Prof. W. A. Miller. *Phil. Mag.* August 1845.

† *Pogg. Ann.* cx. p. 161.

relative position of these bright rays and the dark lines in the lavender part of the spectrum, which are only visible in the sun's light under the most favourable circumstances, must not be relied on as accurate, since it was determined by measurement, and not by direct comparison. Possible inaccuracy of adjustment will render the fourth place of decimals quite uncertain. The remarkable extension of the spectrum of this electric light, both at the red and violet ends, is, however, indisputable.

Electric light.	Sunlight.
Limit of red 1·6025 (about)	Line X 1·6038
— — — — —	„ A 1·6069
— — — — —	„ G 1·6404
Bundle of violet rays ... 1·6436	— — — — —
Faint violet ray 1·6516	„ K 1·6513
2nd „ „ 1·6531 (about)	— — — — —
Bright violet ray 1·6550	„ I 1·6548
Faint ray 1·6562 (about)	— — — — —
„ „ 1·6574 (about)	„ L 1·6567
Limit of violet 1·6664 (about)	„ N (1st) 1·6642

“On Great Fluctuations of Temperature in the Arctic Winter.”
By J. J. Murphy, Esq.

GEOLOGICAL SOCIETY.

[Continued from vol. xxiii. p. 559.]

April 16, 1862.—Prof. Ramsay, President, in the Chair.

The following communications were read:—

1. “On the Position of the *Pteraspis*-beds, and on the Sequence of the Strata of the Old Red Sandstone Series, in South Perthshire.”
By Prof. R. Harkness, F.R.S., F.G.S.

At the Bridge-of-Allan the lowest beds seen of the Old Red Series are (1) conglomerates of trap-rocks, overlain by (2) grey sandstone passing upwards into red sandstone. These grey sandstones have afforded to Mr. Powrie a *Pteraspis* (perhaps *Pt. rostratus*); and fragments of *Cephalaspis* have also been found in them. Next above come (3) purple shales (at Craig Arnhall); then the brown sandstones (4) of Downe Castle; and lastly, at Laurick, grey sandstones (5) again. These all have a N.W. dip, as seen along the Teith. From about Laurick to Callander the strata are best seen in the Keltic burn. They have a S.E. dip, and form the other, but steeper, side of a synclinal trough (about twelve miles wide); and here grey sandstone (5), brown sandstone (4) (at Braeklin Linns), purple shale (3), red and grey sandstone (2), and lastly a conglomerate (1) (here composed of felstone) present an analogous succession to that between Laurick and the Bridge-of-Allan. At Callander the conglomerate lies almost vertically against the metamorphic Lower Silurian rocks of the Grampians, trap-rock intervening. The author estimates that these Old Red strata have a thickness of 7000 feet.

2. "On the Western Extremity of the London Basin; on the Westerly Thinning of the Lower Eocene Beds in that Basin; and on the Greywethers." By William Whitaker, Esq., B.A., F.G.S., of the Geol. Surv. Great Britain.

In the first part of this paper the author described certain outliers of Tertiary strata in the neighbourhood of Bedwin and Savernake (or Marlborough) Forest, in Wiltshire, where in the course of the Geological Survey of the district he found that both the Woolwich and Reading Beds and the London Clay gradually thinned out westward, until merely 3 or 4 inches of the latter alone remained between the Bagshot Beds and the Chalk. Further eastward these are probably in direct apposition. The superficial loam and clay with unworn flints on the Chalk district along the northern side of the London Basin were then described.

In the second portion of the paper it was shown, both from the published results of Mr. Prestwich's researches and later observations made in the progress of the Geological Survey, that the Thanet Sands thin out westwardly, from a thickness of about 85 feet in the Isle of Thanet, to about 35 feet at London, and to 3 feet at Chobham, disappearing altogether near Epsom. The Woolwich and Reading Beds include the Blackheath Pebble-bed, according to the author: at Herne Bay Mr. Whitaker gives these beds a thickness of about 50 feet, at Croydon 45 feet, at New Cross 54 feet, at London from 40 to 70 feet, at Ealing 60 feet, at Hanwell 75 feet, at Chiswick 90 feet, at Reading about 50 feet, and near Great Bedwin in Wiltshire only 15 feet. The London Clay, with its Basement-bed, is nearly 480 feet thick at Sheppey, 400 feet at London, 370 feet at Reading, 20 to 60 feet near Newbury, only 15 feet near Great Bedwin, and is represented by a few inches of its pebbly basement-bed in Marlborough Forest.

The third part of the paper treated of the Greywether Stones of Wiltshire, which the author believes must have come from the Bagshot Sand, which alone of the Tertiary Beds is present there in sufficient thickness to yield these large and numerous masses of bedded rock.

3. "On a Clay-deposit with Insects, Leaves, &c. near Ulverston." By John Bolton, Esq.

The deposit described in this paper was a greenish-drab clay, lying beneath a capping of locally derived drift and rubble of varying thickness, upon the Mountain-limestones of Low Furness. It was met with during the progress of drainage-works undertaken by the Lindal-Cote Iron-ore Company. At one locality, the clay is 93 feet from the surface, and has a thickness of 15 feet; it seems to fill a basin in the limestone. The imbedded plant- and insect-remains and its contained Diatomaceæ proved the deposit to be of lacustrine origin. Fragments of wood occurred in it, stained blue by phosphate of iron. It appeared probable from the depth at which the clay was buried beneath locally derived material, upon a comparatively level surface, that it was of great antiquity, though possibly younger than the glacial epoch.

May 7.—Prof. A. C. Ramsay, President, in the Chair.

The following communications were read:—

1. “Note respecting the Discovery of a new and large *Labyrinthodont* (*Loxomma Allmani*, Huxley) in the Gilmerton Iron-stone of the Edinburgh Coal-field.” By Prof. T. H. Huxley, F.R.S., Sec.G.S.

Looking over the vertebrate fossils from Burdie House and Gilmerton in the University Museum, Edinburgh, Prof. Huxley came upon some reptilian specimens—a fragment of the hinder part of the upper wall of a cranium and some sternal plates of a *Labyrinthodont*, which, from the obliquity of its orbits, he names *Loxomma*. The skull would be about 14 inches long if perfect, and the animal about 6 or 7 feet.

2. “Note on a new *Labyrinthodont* (*Pholidogaster pisciformis*, Huxley) from the Edinburgh Coal-field.” By Prof. T. H. Huxley, F.R.S., Sec.G.S.

The specimen on which this new form has been determined was placed in the British Museum by Sir P. Egerton and Lord Enniskillen, who recognized it as Reptilian. Mr. Davis, of the British Museum, drew Mr. Huxley’s attention to it as being probably *Archegosaurian*. It is not well preserved, but on careful study proves to be an amphibian allied to *Archegosaurus*, differing, however, from it in the form of the head, the extent to which the ossification of the vertebral column has proceeded, and in the character of the dermal armour. This animal was about 44 inches long.

3. “On the Land Flora of the Devonian Period in North-eastern America.” By J. W. Dawson, LL.D., F.G.S.

First noticing what was formerly known of the Devonian Plant-remains in the States of New York and Pennsylvania (Hall, Vanuxem, and Rogers), in Gaspé (Logan), in New Brunswick and Maine (Gesner, Robb, Bennett, Hartt, Matthew, and Hitchcock), the author stated that, with Messrs. Hartt, Matthew, and others at St. John’s, he had lately examined the productive localities near that city, and was now enabled to add largely to the account of the Devonian plants he had already published in the ‘*Canadian Naturalist*,’ vol. vi. 1861. He now enumerates about 70 species (32 genera) of plants as occurring in the Upper Devonian of Pennsylvania, New Brunswick, Maine, New York, and Gaspé, in the Middle Devonian of New York and Gaspé, and in the Lower Devonian of Gaspé. Of these 70 species, two (*Psilophyton princeps* and *Cordaïtes angustifolius*) are referred also to the Upper Silurian of Gaspé; and 10 (not including these two) reappear in the Carboniferous strata. The Devonian Flora much resembles in general facies that of the Carboniferous period. In the Lower Devonian series the underclays are filled with the rhizomes of *Psilophyton*, in the Upper Devonian with *Sigillaria* and *Calamites* (as in the Coal-measures). The Devonian Flora is less perfectly preserved than that of the Coal-measures, and is probably yet very imperfectly known; it presents more resemblance to the floras of the Mesozoic period and of modern

tropical and austral islands than the coal-plants present. The facies of the Devonian Flora in North America is very similar to that of the same period in Europe.

Among the Devonian plants of N.E. America, Dr. Dawson recognizes an angiospermous dicotyledon (*Syringoxylon mirabile*, nov. gen. et sp.), established on a fragment of fossil wood collected by Prof. James Hall from a limestone of the Upper Hamilton Group, at Eighteen-mile Creek on Lake Erie.

4. "On some Upper Eocene Fossils from the Isle of Wight." By Prof. Dr. F. Sandberger. In a Letter to W. J. Hamilton, Esq., For. Sec. G.S.

The result of Prof. Sandberger's examination of a collection of these fossils, carefully named by Mr. F. E. Edwards and forwarded by Mr. Hamilton, has been to confirm him in his opinion that the upper beds at Hempstead, Isle of Wight, are the exact equivalent of the marine beds at Weinheim, Jeurres, and Bergh ("*Rupélien inférieur*" of Dumont). The freshwater limestone of Bembridge and Sconce appears to correspond to the beds at Buxweiler (Alsace) and Abstadt (Baden); and the fossils from Headon Hill and Colwell Bay probably belong to the level of Dumont's "*Tongrien inférieur*" (Lethen and Westergeln) "*et supérieur* (Marnes supérieurs au gypse)."

XII. Intelligence and Miscellaneous Articles.

ON THE ELECTROMOTIVE FORCE OF VOLTAIC PILES..

BY M. MARIE DAVY.

HOWEVER carefully units of resistance and of current are defined, inasmuch as these units are arbitrarily chosen, it cannot be hoped that, in estimating the electromotive force of batteries, they will directly furnish the calorific value of the special work performed by the chemical actions of these batteries. It is necessary to determine the value of a constant numerical coefficient which, like the units of resistance and of current, could be easily obtained by each physicist.

The most simple of all batteries is Smee's. The only chemical action produced normally is, the solution of zinc in acid with disengagement of hydrogen. But the quantity of heat disengaged in this action has been very carefully measured by M. Favre, and fixed by him at 18,444 for ordinary zinc, and at 18,791 for amalgamated; hence I have investigated the numerical coefficient of Smee's battery.

In a preliminary research on this battery, comprising 125 determinations of its electromotive force, I obtained results varying from 16,886 to 20,604. The difference is two-tenths, while my measurements would indicate a thousandth. This research demonstrated the existence of seven disturbing causes:—

1. *Influence of the air dissolved in the acidulated water.*—The electromotive force of a Smee's battery, the acidulated water of which is aerated, decreases gradually as the battery works; it is stationary

at its minimum value when the vacuum is made and the causes indicated below do not intervene.

The oxygen of the air attacks the zinc directly, a corresponding quantity of water escapes decomposition, and the negative work of the reduction of hydrogen is diminished by so much. In one of my experiments the electromotive force of an aërated pile was 19,320, the same pile deprived of air giving 18,796. The difference, 524, corresponds, with a mean current equal to 500, to a consumption of $\frac{1}{100}$ of a milligramme of oxygen during the ten minutes that each of my experiments lasted.

This influence of the air is one of the principal causes of the variations of the constants of the pile, noted successively by MM. Fechner Ohm, Jacobi, Despretz, De la Rive, Poggendorff, and, lastly, by M. Du Moncel. The air only dissolves in limited quantity in water; its relative influence is therefore more marked when, the current being feebler, the battery consumes less. It follows that the electromotive force ought to increase with the resistance of the circuit.

2. *Influence of the sulphate dissolved.*—The laws of the conductivity of saline solutions could alone give me the key to the effects produced by the zinc-salt formed in Smee's battery. The water, acid, and sulphate are all separately conductors; each conducts and is decomposed. The water and the acid concur in producing the normal effect, because both produce hydrogen. This is not the case with the sulphate: zinc is reduced; now this reduction of zinc gives rise to a negative work equal to 53,260 instead of 34,460, the negative work of the reduction of hydrogen. The reduced zinc does not, it is true, usually appear, because as soon as an atom of the free metal touches the platinum, a local couple is produced which redissolves it; but the work thus restored does not improve the general current, which remains diminished.

The electromotive force of a Smee's element with acidulated water being 18,796, was reduced to 18,069 by the solution of 5 decigrammes of pure sulphate of zinc in the liquor. The difference, 727, supposes that the conductivity of acidulated water was twenty-five times as great as that of the dissolved sulphate of zinc.

The influence of the sulphate of zinc is more marked, as this salt is more abundant, and the solution is less acid, and consequently less conducting. Neglecting the influence of the air, it is to the presence of sulphate of zinc that the gradual enfeeblement of Smee's pile is due.

3. *Influence of the concentration of the acid.*—So long as the acid solution contains more than 25 equivalents of water for 1 of acid, the electromotive force remains constant; but when the proportion of acid is greater, the electromotive force is increased by the quantity of heat which would be disengaged by mixing the solution with its complement of water in the proportion of 25 equivalents. When the acid is too concentrated, traces of sulphurous acid may be formed. Thus a Smee's element mounted with concentrated acid, simply diluted with its weight of water, gave 20,279, difference 1483, while, according to M. Favre, this difference ought to be only 743.

4. *Influence of the zinc.*—Amalgamated zinc, as is known, gives stronger batteries than non-amalgamated ones; but the force varies also notably with the state of the amalgam and the purity of the zinc. Commercial zinc amalgamated four days, and presenting a crystalline surface giving 18,796, I found only 18,510 for pure distilled zinc dissolved in pure mercury; difference, 286.

5. *Influence of the purity of the sulphuric acid.*—Ordinary commercial acids contain traces of nitrogen compounds which increase the electromotive force of Smee's battery. Thus with an ordinary acid I obtained 18,961 instead of 18,796 as with a pure acid; difference, 165.

6. *Influence of the water.*—The influence of the water is to me the most inexplicable. While with distilled water I obtain the normal number 18,796, with unfiltered Paris water it was only 16,886; difference, 1910.

7. *Influence of the temperature.*—The influence of temperature on the conductivity of saline solutions and of metals is too marked not to be shown, if not upon the electromotive force of a pile, at least on its numerical estimation. Exact results are not possible if the temperature of the circuit varies in any of its points during the course of an experiment; no comparable results are obtained if these results of the variation of conductivity, due to the changes of temperature from one experiment to another, are not corrected.

In fine, to have concordant results, I work with a Smee's battery consisting of a plate of platinized platinum immersed vertically in a mixture of sulphuric acid with eight or ten times its weight of distilled water boiled free from air. This solution is in a vertical glass tube, at the bottom of which is a liquid amalgam of pure zinc dissolved in pure mercury. A platinum wire traversing the bottom of the tube forms the negative pole of the element. The element is immersed in a large vessel full of water, which keeps its temperature constant; by this arrangement the traces of sulphate formed arrive with difficulty at the platinum: the liquids, moreover, are frequently changed. Porous vessels are a source of trouble. Instead of varying the resistances so as to cause the intensity of the current to oscillate between two constant limits, and measuring by means of the rheostat, according to Wheatstone's method, the variable resistance employed, I introduce in the circuit fixed resistances in platinum, the temperature of which is exactly known, and hence the resistance determined; and I measure the corresponding variable intensities.

I take 18,510 for the electromotive force of this pile, which regulates the value of my constant coefficient.—*Comptes Rendus*, November 4, 1861.

EXAMINATION OF THE WINDOW-GLASS OF POMPEII.

BY M. G. BONTEMPS.

After citing the few references to window-glass to be found in ancient authors, and adverting to the dispute which has prevailed as to the antiquity of the invention of that material, the author quotes

the following passage from the work of M. Mazois, *Les Ruines de Pompeii*, tom. ii. p. 77 :—

“If the question of the employment of window-glass by the ancients were still doubtful, we should find in this hall evidence fitted to solve it; time has spared here a glazed bronze window-frame which determines not only the size and thickness of the glasses employed, but likewise the manner of fitting them; These glasses were placed in a groove and held from place to place by nuts which turned down upon the glasses to fix them; their width is about 20 inches, by 28 inches in height, and their thickness more than 2 lines.”

Having ascertained the employment of window-glass at a period anterior to the year 79 of our era, which is the date of the eruption by which Herculaneum and Pompeii were destroyed, it becomes very interesting to learn how these large panes of glass were manufactured—whether they had been blown in cylinders or in disks, or cast in the manner of plate glass. If blown, they could not have been the product of a single dipping of glass; and in this case the different dippings would be recognizable on the edge of the glass. If they were produced by blowing a cylinder and cutting it open, the bubbles contained in them would be elongated and parallel in the direction of the axis of the cylinder; and they would be concentric if the glass were produced in the manner of crown glass. If the glass were cast, the bubbles would have no uniform direction, and would generally be round and flat. The author obtained some fragments of the glass, measuring not less than 10 centimetres, the examination of which left no doubt as to the mode of their manufacture.

The glass is well fused, free from knots and other defects; some parts are free from bubbles, whilst great numbers occur in other portions, but they are not all due to the fusion. The thickness of the glass is unequal; it is more than 5 millimetres in some places; in others not 3. This alone would indicate that the glass was not blown. One surface bears the impression of the slab on which the glass reposed when hot; this might be the mark of the stone on which the cylinder was opened out; but the other surface does not resemble that produced by blowing: other more certain signs that the glass was not blown are furnished by the bubbles, which are not those of a cylinder, nor of a globe opened out into a disk. Each glass has evidently been cast: the casting in certain parts has not quite reached its proper limit; in others, on the contrary, the workman having arrived near the limit, has turned back by folding the glass upon itself, and thus there has been interposition of air, and formation of a stratum of bubbles. The irregularity of thickness proves that a metallic cylinder was not employed to press upon the glass.

It seems probable therefore that a metal frame of the size of the pane of glass to be produced was placed upon a polished stone, on which a little very finely powdered clay was sprinkled; into this frame was poured glass extracted from the melting pot with bronze ladles or even with tubes, and the glass was pressed with a wooden pallet to make it fill the interior of the frame. The ancients were consequently very near the invention of plate glass, which only took

place in France seventeen centuries later : had they passed a roller over this frame, they would have obtained glass of uniform thickness, and nothing more would have been required except to polish the surfaces, an operation to which they were no strangers ; for Pliny says that they made use of obsidian to make mirrors which they attached to the walls ; and this could only be done when the obsidian was polished.

The Pompeian window-glass is of a bluish-green tint, like our common glass to within the last fifty years. Its analysis by M. F. Claudet gave the following result :—

Silica	69·43
Lime	7·24
Soda	17·31
Alumina	3·55
Oxide of iron	1·15
Oxide of manganese	0·39
Oxide of copper	traces
	<hr/> 99·07

This composition agrees almost exactly with that of the window-glass made now-a-days. The analysis of this given by Dumas shows—

Silica	68·65
Lime	9·65
Soda	17·70
Alumina	4·00

In this, traces of iron and manganese may have been neglected, and the glass was of inferior quality, the average of composition of our window-glass being as follows :—

Silica	72·50
Lime	13·10
Soda	13·00
Alumina	1·00
Oxides of iron and manganese	0·40

—*Comptes Rendus*, May 5, 1862, p. 980.

ON A QUESTION IN THE THEORY OF PROBABILITIES.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

The law of form noticed by Mr. Cayley in the June Number of the *Philosophical Magazine*, p. 471, is true when the data are the probabilities of events unconnected by any conditions expressed or implied ; but it is then true, not as a first principle, but as a consequence of acknowledged principles in the theory of probabilities. As it has not been proved to be true beyond this case (indeed it is certainly not generally true), no argument ought to be built upon the assumption that it is true.

I am, Gentlemen,

Your obedient Servant,

GEORGE BOOLE.

June 11, 1862.

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

AUGUST 1862.

XIII. *On the Application of the Theorem of the Equivalence of Transformations to the Internal Work of a mass of Matter.* By Professor R. CLAUSIUS*.

IN a memoir published in the year 1854†, wherein I sought to simplify to some extent the form of the developments I had previously published, I deduced, from my fundamental proposition *that heat cannot of itself pass from a colder into a warmer body*, a principle which is closely allied to, but does not entirely coincide with, the one first deduced by S. Carnot from considerations of a different class, based upon the older views of the nature of heat. It has reference to the circumstances under which work can be transformed into heat, and, conversely, heat converted into work; and I have called it the *Principle of the Equivalence of Transformations*. I did not, however, there communicate the entire proposition in the general form in which I had deduced it, but confined myself on that occasion to the publication of a part which can be treated separately from the rest, and is capable of more strict proof.

In general, when a body changes its state, work is performed *externally* and *internally* at the same time,—the external work having reference to the forces which extraneous bodies exert upon the body under consideration, and the internal work to the forces exerted by the constituent molecules of the body in question upon each other. The internal work is for the most part so little known, and connected with another equally unknown quan-

* Translated from the *Mittheilungen der Naturforschenden Gesellschaft in Zürich*, vol. vii. p. 48, having been communicated to the Society on the 27th of January, 1862.

† “On a modified form of the second Fundamental Theorem in the Mechanical Theory of Heat” (*Phil. Mag. S. 4.* vol. xii. p. 81; *Pogg. Ann.* vol. xciii. p. 481).

tity in such a way, that in treating of it we are obliged in some measure to trust to probabilities; whereas the external work is immediately accessible to observation and measurement, and thus admits of more strict treatment. Accordingly, since, in my former paper, I wished to avoid everything that was hypothetical, I entirely excluded the internal work, which I was able to do by confining myself to the consideration of *circular processes*—that is to say, operations in which the modifications which the body undergoes are so arranged that the body finally returns to its original condition. In such operations the internal work which is performed during the separate modifications, partly in a positive sense and partly in a negative sense, neutralizes itself, so that nothing but external work remains, for which the principle in question can then be demonstrated with mathematical strictness, starting from the above-mentioned fundamental proposition.

I have delayed till now the publication of the remainder of my theorem, because it leads to a consequence which is considerably at variance with the ideas hitherto generally entertained of the heat contained in bodies, and I therefore thought it desirable to make still further trial of it. But as I have become more and more convinced in the course of years that we must not attach too great weight to such ideas, which in part are founded more upon usage than upon a scientific basis, I feel that I ought to hesitate no longer, but to submit to the scientific public the theorem of the equivalence of transformations in its complete form, with the principles which attach themselves to it. I venture to hope that the importance which these principles, supposing them to be true, possess in connexion with the theory of heat will be thought to justify their publication in their present hypothetical form.

I will, however, at once distinctly observe that, whatever hesitation may be felt in admitting the truth of the following principles, the conclusions arrived at in my former paper, in reference to circular processes, lose thereby none of their authority.

§ 1. I will begin by briefly stating the principle of the equivalence of transformations, as I have already developed it, in order to be able to connect with it the following considerations.

When a body goes through a circular process, a certain amount of external work may be gained, in which case a certain quantity of heat must be simultaneously expended; or, conversely, work may be expended and a corresponding quantity of heat may be gained. This may be expressed by saying:—*Heat can be transformed into work, or work into heat, by a circular process.*

There may also be another effect of a circular process: heat may be transferred from one body to another, by the body which

is undergoing modification absorbing heat from the one body and giving it out again to the other. In this case the bodies between which the transference of heat takes place are to be viewed merely as heat-reservoirs, of which we are not concerned to know anything except the temperatures. If the temperatures of the two bodies differ, heat passes, either from a warmer to a colder body, or from a colder to a warmer body, according to the direction in which the transference of heat takes place. Such a passage of heat may also be designated, for the sake of uniformity, as a *transformation*, inasmuch as it may be said that *heat of one temperature is transformed into heat of another temperature*.

The two kinds of transformations that have been mentioned are related in such a way that one presupposes the other, and that they can mutually replace each other. If we call transformations which can replace each other *equivalent*, and seek the mathematical expressions which determine the amount of the transformations in such a manner that equivalent transformations become equal in magnitude, we arrive at the following expression:—*If the quantity of heat Q of the temperature t is produced from work, the equivalent value of this transformation is*

$$\frac{Q}{T};$$

and if the quantity of heat Q passes from a body whose temperature is t_1 into another whose temperature is t_2 , the equivalent value of this transformation is

$$Q \left(\frac{1}{T_2} - \frac{1}{T_1} \right),$$

where T is a function of the temperature which is independent of the kind of process by means of which the transformation is effected, and T_1 and T_2 denote the values of this function which correspond to the temperatures t_1 and t_2 . I have shown by separate considerations that T is in all probability nothing more than the absolute temperature.

These two expressions further enable us to recognize the positive or negative sense of the transformations. In the first, Q is taken as positive when work is transformed into heat, and as negative when heat is transformed into work. In the second, we may always take Q as positive, since the opposite senses of the transformations are indicated by the possibility of the difference $\frac{1}{T_2} - \frac{1}{T_1}$ being either positive or negative. It will thus be

seen that the passage of heat from a higher to a lower temperature is to be looked upon as a positive transformation, and its

passage from a lower to a higher temperature as a negative transformation.

If we represent the transformations which occur in a circular process by these expressions, the relation existing between them can be stated in a simple and definite manner. If the circular process is *reversible*, the transformations which occur therein must be partly positive and partly negative, and the equivalent values of the positive transformations must be together equal to those of the negative transformations, so that the algebraic sum of all the equivalent values becomes $=0$. If the circular process is *not reversible*, the equivalent values of the positive and negative transformations are not necessarily equal, but they can only differ in such a way that the positive transformations predominate. The proposition respecting the equivalent values of the transformations may accordingly be stated thus:—*The algebraic sum of all the transformations occurring in a circular process can only be positive, or, as an extreme case, equal to nothing.*

The mathematical expression for this proposition is as follows. Let dQ be an element of the heat given up by the body to any reservoir of heat during its modifications (heat which it may absorb from a reservoir being here reckoned as negative), and T the absolute temperature of the body at the moment of giving up this heat, then the equation

$$\int \frac{dQ}{T} = 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (I.)$$

must be true for every reversible circular process, and the relation

$$\int \frac{dQ}{T} \geq 0 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (Ia.)$$

must hold good for every circular process which is in any way possible.

§ 2. Although the necessity of this proposition admits of strict mathematical proof if we start from the fundamental principle above quoted, it thereby nevertheless retains an abstract form, in which it is difficultly embraced by the mind, and we feel compelled to seek for the precise physical cause, of which this proposition is a consequence. Moreover, since there is no essential difference between internal and external work, we may assume almost with certainty that a proposition which is so generally applicable to external work cannot be restricted to this alone, but that, where external work is combined with internal work, it must be capable of application to the latter also.

Considerations of this nature led me, in my first investigations into the mechanical theory of heat, to assume a general law respecting the dependence of the active force of heat on tempe-

perature, among the immediate consequences of which is the principle of the equivalence of transformations in its more complete form, and which at the same time involves other important conclusions. This law I will at once quote, and will endeavour to make its meaning clear by the addition of a few comments. As for the reasons for supposing it to be true, such as do not at once appear from its internal probability will gradually become apparent in the course of this paper. It is as follows:—

In all cases in which the heat contained in a body does mechanical work by overcoming a resistance, the magnitude of the resistance which it is capable of overcoming is proportional to the absolute temperature.

In order to understand the significance of this law, we require to consider more closely the processes by which heat can perform mechanical work. These processes always admit of being reduced to the alteration in some way or another of the arrangement of the constituent molecules of a body. For instance, bodies are expanded by heat, their molecules being thus separated from each other: in this case the mutual attractions of the molecules on the one hand, and on the other external opposing forces, in so far as any such are in operation, have to be overcome. Again, the state of aggregation of bodies is altered by heat, solid bodies being rendered liquid, and both solid and liquid bodies being rendered aëriform: here likewise internal forces, and in general external forces also, have to be overcome. Another case which I will also mention, because it differs so widely from the foregoing, and therefore shows how various are the modes of action which belong to the class we are considering, is the transference of electricity from one body to the other, constituting the thermoelectric current, which takes place by the action of heat on two heterogeneous bodies in contact.

In the cases first mentioned, the arrangement of the molecules is altered. Since, even while a body remains in the same state of aggregation, its molecules do not retain fixed unvarying positions, but are constantly in a state of more or less extended motion, we may, when speaking of the *arrangement of the molecules* at any particular time, understand either the arrangement which would result from the molecules being fixed in the actual positions they occupy at the instant in question, or we may suppose such an arrangement that each molecule occupies its mean position. Now the effect of heat always tends to loosen the connexion between the molecules, and so to increase their mean distances from one another. In order to be able to represent this mathematically, we will express the degree in which the molecules of a body are dispersed, by introducing a new magnitude, which we will call the *disgregation* of the body, and by help

of which we can define the effect of heat as simply *tending to increase the disgregation*. The way in which a definite measure of this magnitude can be arrived at will appear from the sequel.

In the case last mentioned, an alteration in the arrangement of the electricity takes place, an alteration which can be represented and taken into calculation in a way corresponding to the alteration of the position of the molecules, and which, when it occurs, we will consider as always included in the general expression *alteration of arrangement, or alteration of disgregation*.

It is evident that each of the kinds of alteration that have been named may also take place in the reverse sense, if the effect of the opposing forces is greater than that of the heat. We will assume as likewise self-evident that, for the production of work, a corresponding quantity of heat must always be expended, and conversely, that, by the consumption of work, an equivalent quantity of heat must be produced.

§ 3. If we now consider more closely the various cases which occur in relation to the forces which are operative in each of them, the case of the expansion of a permanent gas presents itself as particularly simple. We may conclude from certain properties of the gases that the mutual attraction of their molecules at their mean distances is very small, and therefore that only a very slight resistance is offered to the expansion of a gas, so that the resistance of the sides of the containing vessel must maintain equilibrium with almost the whole effect of the heat. Accordingly the externally sensible pressure of a gas forms an approximate measure of the separative force of the heat contained in the gas; and hence, according to the foregoing proposition, this pressure must be nearly proportional to the absolute temperature. The internal probability of the truth of this result is indeed so great, that many physicists since Gay-Lussac and Dalton have without hesitation presupposed this proportionality, and have employed it for calculating the absolute temperature.

In the above-mentioned case of thermo-electric action, the force which exerts an action contrary to that of the heat is likewise simple and easily determined. For at the point of contact of two heterogeneous substances, such a quantity of electricity is driven from the one to the other by the action of the heat, that the opposing force resulting from the electric tension suffices to hold the force exerted by the heat in equilibrium. Now in a former memoir "*On the application of the Mechanical Theory of Heat to the Phenomena of Thermal Electricity**," I have shown that, in so far as changes in the arrangement of the molecules are not necessarily produced at the same time by changes of temperature, the difference of tension produced by heat must be proportional

* Poggendorff's *Annalen*, vol. xc. p. 513.

to the absolute temperature, as is required by the foregoing theorem.

In the other cases that are quoted, as well as in most others, the relations are less simple, because in them an essential part is played by the forces exerted by the molecules upon one another, forces which, as yet, are quite unknown. It results, however, from the mere consideration of the external resistances which heat is capable of overcoming, that in general its force increases with the temperature. If we wish, for instance, to prevent the expansion of a body by means of external pressure, we are obliged to employ a greater pressure the more the body is heated; hence we may conclude, without having a knowledge of the internal forces, that the total amount of the resistances which can be overcome in expansion, increases with the temperature. We cannot, however, directly ascertain whether it increases exactly in the proportion required by the foregoing theorem, without knowing the internal forces. On the other hand, if this theorem be regarded as proved on other grounds, we may reverse the process, and employ it for the determination of the internal forces exerted by the molecules.

The forces exerted upon one another by the molecules are not of so simple a kind that each molecule can be replaced by a mere point; for many cases occur in which it can be easily seen that we have not merely to consider the distances of the molecules, but also their relative positions. If we take, for example, the melting of ice, there is no doubt that here internal forces, exerted by the molecules upon each other, are overcome, and accordingly disgregation takes place; nevertheless the centres of gravity of the molecules are on the average not so far removed from each other in the liquid water as they were in the ice, for the water is the more dense of the two. Again, the peculiar behaviour of water in contracting when heated above 0° C., and only beginning to expand when its temperature exceeds 4° , shows that likewise in liquid water, in the neighbourhood of its melting-point, increase of disgregation is not connected with increase of the mean distances of its molecules. In the case of the internal forces, it would accordingly be difficult—even if we did not want to measure them, but only to represent them mathematically—to find a fitting expression for them which would admit of a simple determination of magnitude. This difficulty, however, disappears if we take into calculation, not the forces themselves, but the *mechanical work* which in any alteration of arrangement is required to overcome them. The expressions for the quantities of work are simpler than those for the corresponding forces; for the quantities of work can be all expressed, without further secondary statements, by numbers with the same unit, which can be added

together, or subtracted from one another, however various the forces may be to which they are referable.

It is therefore convenient to alter the form of the above theorem by introducing, instead of the forces themselves, the work done in overcoming them. In this form it reads as follows:—

The mechanical work which can be exerted by heat in any alteration of the arrangement of a body is proportional to the absolute temperature at which this alteration occurs.

§ 4. The theorem does not speak of the work which the heat *does*, but of the work which it *can do*; and similarly, in the first form of the theorem, the resistances which the heat *can overcome* are spoken of. This distinction is necessary for the following reasons.

Since the external forces which act upon a body while it is undergoing an alteration of arrangement may vary very greatly, it may happen that the heat, while causing an alteration of arrangement, has not to overcome the whole resistance which it would be possible for it to overcome. A well-known and often-quoted example of this is afforded by a gas which expands under such conditions that it has not to overcome an opposing pressure equal to its own expansive force, as, for instance, when the space filled by the gas is made to communicate with another which is empty, or contains a gas of lower pressure. In order in such cases to determine the force of the heat, we must evidently not consider the resistance which actually is overcome, but that which can be overcome.

Also in alterations of arrangement of the opposite kind, that is, where the action of heat is overcome by the opposing forces, a similar distinction may require to be made, but in this case only as far as this—that the total amount of the forces by which the action of the heat is overcome may be greater than the active force of the heat, but not smaller.

Cases in which these differences occur may be thus characterized. When an alteration of arrangement takes place so that the force and counterforce are equal, the alteration can take place in the reverse direction also under the influence of the same forces. But if it occurs so that the overcoming force is greater than that which is overcome, the transformation cannot take place in the opposite direction under the influence of the same forces. We may say that the transformation has occurred in the first case in a *reversible* manner, and in the second case in an *irreversible* manner.

Strictly speaking, the overcoming force must always be more powerful than the force which it overcomes; but as the excess of force does not require to have any assignable value, we may think of it as becoming continually smaller and smaller, so that

its value may approach to nought as nearly as we please. Hence it may be seen that the case in which the transformations take place reversibly is a limit which in reality is never quite reached, but to which we can approach as nearly as we please. We may therefore, in theoretical discussions, still speak of this case as one which really exists; indeed, as a limiting case it possesses special theoretical importance.

I will take this opportunity of mentioning another process in which likewise this distinction is to be observed. In order for one body to impart heat to another by conduction or radiation (in the case of radiation, wherein mutual communication of heat takes place, it is to be understood that we speak here of a body which gives out more heat than it receives), the body which parts with heat must be warmer than the body which takes up heat; and hence the passage of heat between two bodies of different temperature can take place in one direction only, and not in the contrary direction. The only case in which the passage of heat can occur equally in both directions is when it takes place between bodies of equal temperature. Strictly speaking, however, the communication of heat from one body to another of the same temperature is not possible; but since the difference of temperature may be as small as we please, the case in which it is equal to nothing, and the passage of heat accordingly reversible, is a limiting case which may be regarded as theoretically possible.

§ 5. We will now deduce the mathematical expression for the above theorem, treating in the first place the case in which the change of condition undergone by the body under consideration takes place *reversibly*. The result at which we shall arrive for this case will easily admit of subsequent generalization, so as to include also the cases in which an alteration occurs irreversibly.

Let the body be supposed to undergo an infinitely small alteration of condition, whereby the quantity of heat contained in it, and also the arrangement of its constituent molecules, may be altered. Let the quantity of heat contained in it be expressed by H , and the alteration of this quantity by dH . Further, let the work, both internal and external together, performed by the heat in the change of arrangement be denoted by dL , a magnitude which may be either positive or negative according as the active force of the heat overcomes the forces acting in the contrary direction, or is overcome by them. We obtain the heat expended to produce this quantity of work by multiplying the work by the heat-equivalent of a unit of work which we may call A ; hence it is AdL .

The sum $dH + AdL$ is the quantity of heat which the body must receive from without, and must accordingly withdraw from

another body during the alteration of condition. We have, however, already represented by dQ the infinitely small quantity of heat imparted to another body by the one which is undergoing modification, hence we must represent in a corresponding manner, by $-dQ$, the heat which it withdraws from another body. We thus obtain the equation

$$-dQ = dH + AdL,$$

or

$$dQ + dH + AdL = 0^*. \quad . \quad . \quad . \quad . \quad (1)$$

In order now to be able to introduce the disgregation also into the formulæ, we must first settle how we are to determine it as a mathematical quantity.

* In my previous memoirs I have separated from one another the *internal* and the *external* work performed by the heat during the change of condition of the body. If the former be denoted by dI , and the latter by dW , the above equation becomes

$$dQ + dH + AdI + AdW = 0. \quad . \quad . \quad . \quad . \quad . \quad (a)$$

Since, however, the increase in the quantity of heat actually contained in a body, and the heat consumed by internal work during an alteration of condition, are magnitudes of which we commonly do not know the individual values, but only the sum of those values, and which resemble each other in being fully determined as soon as we know the initial and final conditions of the body, without our requiring to know how it has passed from the one to the other, I have thought it advisable to introduce a function which shall represent the sum of these two magnitudes, and which I have denoted by U . Accordingly

$$dU = dH + AdI, \quad . \quad . \quad . \quad . \quad . \quad . \quad (b)$$

and hence the foregoing equation becomes

$$dQ + dU + AdW = 0; \quad . \quad . \quad . \quad . \quad . \quad . \quad (c)$$

and if we suppose the last equation integrated for any finite alteration of condition, we have

$$Q + U + AW = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (d)$$

These are the equations which I have used in my memoirs published in 1850 and in 1854, partly in the particular form which they assume for the permanent gases, and partly in the general form in which they are here given, with no other difference than that I there took the positive and negative quantities of heat in the opposite sense to what I have done here, in order to attain greater correspondence with the equation (I.) given in § 1. The function U which I introduced is capable of manifold application in the theory of heat, and, since its introduction, has been the subject of very interesting mathematical developments by W. Thomson and by Kirchhoff (see *Philosophical Magazine*, S. 4. vol. ix. p. 523, and Poggendorff's *Annalen*, vol. ciii. p. 177). Thomson has called it "the mechanical energy of a body in a given state," and Kirchhoff "Wirkungsfunktion." Although I consider my original definition of it (see *Pogg. Ann.* vol. lxxix. p. 385, and vol. xciii. p. 484), as representing the *sum of the heat added to the quantity already present and of that expended in internal work*, starting from any given initial state, as perfectly exact, I can still have no objection to make against an abbreviated mode of expression.

By disgregation is represented, as stated in § 2, the degree of dispersion of the body. Thus, for example, the disgregation of a body is greater in the liquid state than in the solid, and greater in the aëriform than in the liquid state. Further, if part of a given quantity of matter is solid and the rest liquid, the disgregation is greater the greater the proportion of the whole mass that is liquid; and similarly, if one part is liquid and the remainder aëriform, the disgregation is greater the larger the aëriform portion. The disgregation of a body is fully determined when the arrangement of its constituent molecules is given; but, on the other hand, we cannot say conversely that the arrangement of the constituent molecules is determined when the magnitude of the disgregation is known. It might, for example, happen that the disgregation of a given quantity of matter should be the same when one part was solid and one part aëriform, as when the whole mass was liquid.

We will now suppose that, with the aid of heat, the body changes its state, and we will provisionally confine ourselves to such changes of state as can occur in a constant and reversible manner, and we will also assume that the body has a uniform temperature throughout. Since the increase of disgregation is the action by means of which heat performs work, it follows that the quantity of work must bear a definite ratio to the quantity by which the disgregation is increased; we will therefore fix the still arbitrary determination of the magnitude of disgregation so that, at any given temperature, the increase of disgregation shall be proportional to the work which the heat could perform at that temperature. All that further regards the influence of temperature is determined by the foregoing theorem. For if the same alteration of disgregation takes place at different temperatures, the corresponding work must be proportional to the absolute temperature. Accordingly, let Z be the disgregation of the body, and dZ an infinitely small alteration of it, and let dL be the corresponding infinitely small quantity of work, we can then put

$$dL = KTdZ,$$

or

$$dZ = \frac{dL}{KT},$$

where K is a constant dependent on the unit, hitherto left undecided upon, according to which Z is to be measured. We will choose this unit of measure so that $K = \frac{1}{A}$, and the equation becomes

$$dZ = \frac{AdL}{T} \dots \dots \dots (2)$$

If we suppose this expression integrated, starting with any initial condition in which Z has the value Z_0 , we get

$$Z = Z_0 + A \int \frac{dL}{T}. \quad . \quad . \quad . \quad . \quad . \quad (3)$$

The magnitude Z is thus determined, with the exception of a constant dependent upon the initial condition that is chosen.

If the temperature of the body is not the same at every part, we can regard it as divided into any number we choose of separate parts, and let the elements dZ and dL in equation (2) refer to any one of them, and at once substitute for T the value of the absolute temperature of that part. If we then unite by summation the infinitely small changes of disgregation of the separate parts, or by integration, if there is an infinite number of them, we obtain the similarly infinitely small change of disgregation of the entire body, and from this we can obtain, likewise by integration, any desired finite change of disgregation.

We will now return to equation (1), and by help of equation (2) we will eliminate from it the element of work dL . Thus we get

$$dQ + dH + TdZ = 0; \quad . \quad . \quad . \quad . \quad . \quad (4)$$

or, dividing by T ,

$$\frac{dQ + dH}{T} + dZ = 0. \quad . \quad . \quad . \quad . \quad . \quad (5)$$

If we suppose this equation integrated for a finite change of condition, we have

$$\int \frac{dQ + dH}{T} + \int dZ = 0. \quad . \quad . \quad . \quad . \quad (II.)$$

Supposing the body not to be of uniform temperature throughout, we may imagine it broken up again into separate parts, and can make the elements dQ , dH , and dZ in equation (5) refer in the first instance to one part, and for T we can put the absolute temperature of this part. The symbols of integration in (II.) are then to be understood as embracing the alterations of all the parts. We must here remark that cases in which one continuous body is of different temperatures at different parts, so that a passage of heat immediately takes place by conduction from the warmer to the colder parts, must be for the present disregarded, because such a passage of heat is not reversible, and we have provisionally confined ourselves to the consideration of reversible alterations.

Equation (II.) is the mathematical expression of the above theorem for which we have been seeking, *for all reversible alterations of condition of a body*; and it is clearly evident that it also

remains applicable, if a series of successive alterations of condition be considered instead of a single one.

§ 6. The differential equation (4), whence equation (II.) is derived, is connected with a differential equation which results from the already known principles of the mechanical theory of heat, and which transforms itself directly into (4) for the particular case in which the body under consideration is a perfect gas.

We will suppose that there is given any body of variable volume, acted upon, as by an external force, by the pressure exerted on the surface. Let the volume which it assumes under this pressure, p , at the temperature T (reckoned from the absolute zero) be v , and let it be supposed that the condition of the body is fully determined by the magnitudes T and v . If we now denote by $\frac{dQ}{dv} dv$ the quantity of heat which the body must take up in order to expand to the extent of dv , without alteration of temperature (for the sake of conformity with the mode in which the signs are used in the other equations occurring in this section, the positive sense of the quantity of heat is here taken differently from what it is in equation (4), in which heat given up by the body, and not heat communicated to it, is reckoned positive), the following well-known equation, from the mechanical theory of heat, will hold good:—

$$\frac{dQ}{dv} = AT \frac{dp}{dT}.$$

Let us now suppose that the temperature of the body is changed by dT , and its volume by dv , and let us call the quantity of heat which it then takes up dQ ; we may then write

$$dQ = \frac{dQ}{dT} dT + \frac{dQ}{dv} dv.$$

For the magnitude here denoted by $\frac{dQ}{dT}$, which represents the specific heat with constant volume, we can put the letter c , and for $\frac{dQ}{dv}$ the expression already given. Then we have

$$dQ = cdT + AT \frac{dp}{dT} dv. \quad . \quad . \quad . \quad . \quad (6)$$

The only external force, which the body has to overcome on expanding, being p , the work which it performs in so doing is $p dv$, and the magnitude $\frac{dp}{dT} dv$ indicates the increase of this work with the temperature.

If we now apply this equation to a perfect gas, the specific heat under constant volume is in this case to be regarded as the true specific heat, and this gives the increase in the quantity of heat actually present in the gas; for here no heat is consumed in work, since external work is only performed when increase of volume occurs, and internal work has no existence in the case of perfect gases. We may therefore regard cdT as identical with dH . We have further, for the perfect gases, the equation

$$pc = RT,$$

where R is a constant, and thence we get

$$\frac{dp}{dT} dc = \frac{R}{v} dv = R d \cdot \log v.$$

Equation (6) is thus transformed into

$$dQ = dH + ART d \cdot \log v. \quad . \quad . \quad . \quad . \quad (7)$$

This equation agrees, disregarding the difference in the sign of dQ (which is caused only by the different way in which we have chosen to employ the signs $+$ and $-$ in this case), with equation (4), and the function there represented by the general symbol Z has, in this particular case, the form $AR \log v$.

Rankine, who has written several interesting memoirs on the transformation of heat into work*, has in like manner proposed to transform equation (6), which in its original form applies to perfect gases only†, so as to render it applicable to other bodies, and writes (only with slightly different letters)

$$dQ = kdT + ATdF, \quad . \quad . \quad . \quad . \quad . \quad (8)$$

where k denotes the true specific heat of the body, and F is a magnitude in the determination of which Rankine appears to have been led chiefly by the circumstance mentioned above, that

the quantity $\frac{dp}{dT} dv$ which occurs in equation (6) represents the increase of external work which accompanies an infinitely small alteration of state under increased temperature. Rankine defines the magnitude F as "the rate of variation of effective work with temperature;" and denoting the external work which the body can do in passing, at a given temperature, from a given former state into its present condition, by U , he puts

$$F = \frac{dU}{dT}. \quad . \quad . \quad . \quad . \quad . \quad (9)$$

In the discussion which immediately follows, of the case in which

* Philosophical Magazine, S. 4. vol. v. p. 106; Edinburgh New Philosophical Journal, vol. ii. p. 120; Manual of the Steam-engine.

† Manual of the Steam-engine, p. 310.

the external work consists only in overcoming an external pressure, he gives the equation

$$U = \int p dv,$$

whence follows

$$F = \int \frac{dp}{dT} dv. \quad . \quad . \quad . \quad . \quad . \quad (10)$$

The integrals which here occur are to be taken from a given initial volume to the actually existing volume, the temperature being supposed constant. Introducing this value of F into equation (8), he writes it in the following form :—

$$dQ = \left(k + AT \int_{\infty}^v \frac{d^2 p}{dT^2} dv \right) dT + AT \frac{dp}{dT} dv. \quad . \quad (11)$$

His reason for taking an infinitely large volume as the initial volume is not stated, although the choice of the initial volume is evidently not indifferent.

It is easy to see that this manner of modifying equation (6) is very different from my development ; the results are also discordant ; for the quantity F is not identical with the corresponding $\frac{1}{A} Z$ in my equations, but only coincides with it in that part which could be deduced from data already known ; that is to say, the last member of equation (6) gives the differential coefficient v for the magnitude which has to be introduced, since, to get the correct value of this member, we must in any case put

$$\frac{dF}{dv} = \frac{1}{A} \frac{dZ}{dv} = \frac{dp}{dT}. \quad . \quad . \quad . \quad . \quad (12)$$

Rankine has, however, as may be seen from equation (10), formed the magnitude F by simply integrating according to v the expression given for the differential coefficient according to v . In order to see in what way the magnitude $\frac{1}{A} Z$ differs from this, we will modify somewhat the expression for Z given in the preceding section.

According to equation (2),

$$\frac{T}{A} dZ = dL.$$

dL denotes here the internal and external work, taken together, which is performed when the body undergoes an infinitely slight change of condition. We will denote the internal work by dI ; and since, when the condition of the body is determined by its temperature T and its volume v , I must be a function of these

two quantities, we may write

$$dI = \frac{dI}{dT} dT + \frac{dI}{dv} dv.$$

The external work, assuming it to consist merely in overcoming an external pressure, is represented by $p dv$. Hence, if we further decompose the differential dZ into its two parts, we may write the above equation thus:—

$$\frac{T}{A} \frac{dZ}{dT} dT + \frac{T}{A} \frac{dZ}{dv} dv = \frac{dI}{dT} dT + \left(\frac{dI}{dv} + p \right) dv,$$

whence we have

$$\left. \begin{aligned} \frac{T}{A} \frac{dZ}{dT} &= \frac{dI}{dT}, \\ \frac{T}{A} \frac{dZ}{dv} &= \frac{dI}{dv} + p. \end{aligned} \right\} \dots \dots \dots (13)$$

Differentiating the first of these equations according to v , and the second according to T , we get

$$\begin{aligned} \frac{T}{A} \frac{d^2 Z}{dT dv} &= \frac{d^2 I}{dT dv}, \\ \frac{1}{A} \frac{dZ}{dv} + \frac{T}{A} \frac{d^2 Z}{dT dv} &= \frac{d^2 I}{dT dv} = \frac{dp}{dT}. \end{aligned}$$

The first of these equations subtracted from the second, gives

$$\frac{1}{A} \frac{dZ}{dv} = \frac{dp}{dT}.$$

The differential coefficient of Z according to v consequently fulfils the condition given in (12); the second of the equations (13) gives at the same time the differential coefficient according to T ; and putting these two together, we obtain the complete differential equation

$$\frac{1}{A} dZ = \frac{1}{T} \frac{dI}{dT} dT + \frac{dp}{dT} dv. \quad \dots \dots \dots (14)$$

To obtain the quantity $\frac{1}{A} Z$, we must integrate this equation.

It is easy to see that this integral will in general differ by a function of T from that which would be obtained by integrating only the last term. The two integrals can only be regarded as directly equal if $\frac{dI}{dT} = 0$, whence also, in order that the foregoing equation may be integrable, it follows that $\frac{d^2 p}{dT^2} = 0$, a case which occurs in perfect gases.

I believe that what I can claim as new in my equation (II.) is just this, that the magnitude Z which there occurs has acquired, through my developments, a definite physical meaning, whence it follows that it is fully determined by the arrangement of the constituent molecules of the body existing at any given instant. Thus only does it become possible to deduce from this equation the important conclusion which follows.

[To be continued.]

XIV. *On the Cosmical Features of Terrestrial Magnetism.* By
EDWARD SABINE, R.A., D.C.L. Oxford, and LL.D. Cambridge,
President of the Royal Society.

[The Reade Lecture, delivered in the Senate House of the University of
Cambridge, in May 1862.]

IN directing our attention to the subject of terrestrial magnetism, we may conveniently separate the phenomena into two distinct classes,—viz. those which originate in and indicate the magnetic state of our own planet, and those which admit of a traceable connexion with bodies exterior to the earth, or forces emanating from them. The title of the lecture which I shall have the honour of addressing to you will lead me to dwell chiefly on the second of these two classes of phenomena, namely, on those which with more or less certainty indicate cosmical relations. We are now justified in predicating that such cosmical relations are discernible in all, or almost all, those fluctuations and changes of direction which, as many of my present audience are aware, our magnetic needles are continually undergoing, whether they appear in the form of momentary affections or in regularly recurring periods, such as a day, a month, a year, or a cycle of years.

The advances which have been made in this department of magnetical science date chiefly from the epoch (in 1840) when, on the joint recommendation of the Royal Society and of the British Association for the Advancement of Science, observatories were established in several parts of the globe devoted to magnetic phenomena as their especial and primary object. Prior to this epoch, the knowledge possessed of the periodical magnetic variations was in the highest degree vague, desultory, and inconclusive; whilst the attempts which had been occasionally made to systematize or explain them were either wholly conjectural, or were based on an assumed connexion between the variations of temperature and of magnetism, in which the consistency of the hypotheses with the few facts then known respecting the mag-

Phil. Mag. S. 4. Vol. 24. No. 159. Aug. 1862. H

netic variations was but too lightly regarded. For some years previous to the epoch to which I have referred, the minds of several of the most eminent scientific men on the Continent, as well as in this country, had been impressed with a conviction that it was not altogether creditable that a force should be admitted to exist and to prevail at all parts of the surface of the globe, of the source or of the nature of which, or of its laws of action, we had no proper knowledge. At the instigation of two of the most influential men of the age to which they belonged, MM. de Humboldt and Gauss, Berlin and subsequently Göttingen became successively centres of researches, in which many German observers, incited by the opinions and example of those whom they regarded as scientific leaders, took part. The interest created by these researches, limited as they were both in respect to objects and to the extent of country over which they were made, may be justly referred to as having prepared the way for that more extended and complete organization which, under the auspices of the British Government and the supervision of a Committee of the Royal Society, has succeeded in definitively raising terrestrial magnetism to the dignity of an inductive science, pursuing its steady progress by well-considered and carefully-executed experimental researches, and resting its conclusions on copious and extensive induction.

The attention of the German observers had been directed chiefly to investigations connected with a class of phenomena apparently of a very irregular and almost, as it might have been supposed, of a capricious nature. The magnetic needle was known to be occasionally subject to sudden and transient fluctuations, continuing sometimes for hours and sometimes even for days together, and frequently exceeding in amount of disturbance any known periodical variations. It was found by contemporaneous observations, made on concerted days at several stations in Germany, that these fluctuations occurred simultaneously, and nearly to the same amount, over the whole extent of country in which the "term observations," as they were called, were made, comprising a considerable portion of the north of Germany. It had been at first supposed that these irregular and sudden fluctuations might originate in disturbances of the atmosphere; but the large extent of country over which they were now shown to be synchronous, and even surprisingly similar, not only in the larger but also in most of the smaller oscillations, was unfavourable to this supposition. The result of the German researches was therefore greatly to enhance the interest of these phenomena; but both their source and their laws still remained wholly unknown.

In directing the attention of those who should engage in the

British system of observation to the modes in which the investigations that had been thus commenced could be most successfully prosecuted, the Report of the Royal Society manifested a sagacity which subsequent experience has so fully confirmed that we may, with propriety, on the present occasion recall one or two sentences of that Report, because they strike the key-note of all the discoveries which have since been made, and also because to this day they appear to be too little understood, or at least too little regarded, by many who profess to take part in a system of observation which has accomplished so much for this branch of science. The extracts will be very brief.

“All the magnetic elements are at each point now ascertained to be in a constant state of fluctuation, and affected by transient and irregular changes ; and the investigation of the laws, extent, and mutual relations of those changes is now become essential to the successful prosecution of magnetic discovery, for the following reasons :—

“1st. That the progressive and periodical changes are so mixed up with the transitory changes that it is impossible to separate them so as to obtain a correct knowledge and analysis of the progressive and periodical, without taking express account of and eliminating the transient and irregular ; . . . and further, because the theory of these transitory changes is in itself one of the most interesting and important points to which the attention of magnetic observers can be turned, as they are no doubt intimately connected with the general causes of terrestrial magnetism, and will probably lead us to a more perfect knowledge of those causes than we now possess.”

The interest with which these fluctuations were regarded at the close of the German Association, as shown by the preceding extracts, was much increased by the results of the first year of the observations made, on days concerted with European observatories, at the British Colonial stations in Canada, St. Helena, the Cape of Good Hope, and Van Diemen Island. By the inter-comparison of these it was found that the disturbances were contemporaneous, not only over a limited area in Europe, as previously known, but also at all these widely distributed stations situated in parts of the globe most distant from each other. They were obviously affections common to the whole surface of the earth ; and thus the improbability of their having a local or atmospheric origin was strengthened and confirmed. Whilst, however, evidence was conclusive in regard to their general contemporaneous occurrence, and whilst fluctuations of the most marked character were noticed to be strictly synchronous in the northern and southern hemispheres as well as in Europe and America, it was also found that at stations remote from each

other the disturbance of the same element might differ widely in amount, and might occasionally be even reversed in direction. Not unfrequently also a disturbance showing itself at the same instant at distant stations would affect one magnetic element at one station and another element at another station, thus giving increased probability to a surmise originally due to the sagacity of M. Gauss, that "various forces might not improbably be contemporaneously in action, having possibly very different sources, and proceeding independently of each other, the effects of these various forces being intermixed in very different proportions at various places of observation according to the directions and distances of these from the sources from whence the perturbations proceed."

The problem of the origin and laws of the phenomena might thus be deemed even more complex than had been at first imagined. But in every case the solution required as the first step the separation, at a particular station where observations had been made hourly, of a sufficient body of the disturbed observations to enable their laws to be investigated, should such laws exist. The difficulty which here presented itself was to decide on some criterion by which a disturbed observation might be distinguished from one which was not disturbed. It has been already noticed that the fluctuations which characterize the disturbances are frequently found to exceed the limits of any of the known periodical variations. The *magnitude* of the discordance of an observation, when compared with the mean or normal position of the magnet in the same month and at the same hour, was therefore assumed as a criterion of disturbance for a first essay. Care was taken that the magnitude of disturbance, measured from the mean or normal position, should exceed the probable limit of irregularities which might be occasioned on particular days by the periodical variations of known amount, so that the separated observations might, as far as possible, consist only of those rendered discordant by the special class of phenomena which it was desired to investigate; and the amount of discordance thus taken as characterizing a disturbed observation was made constant for the particular station, in order that the aggregate amounts of disturbance shown by the disturbed observations in the several hours, days, months, and years at that station might, as far as possible, be strictly comparable *inter se*.

The success of the experiment thus conducted was immediate and decisive. A single twelvemonth sufficed to manifest an order and sequence in the ratios of the aggregate amounts of disturbance at each of the twenty-four hours to the mean amount in the twenty-four hours taken as unity, which placed beyond a doubt the fact that, casual and irregular as the disturbances appeared

in respect to the particular times of their occurrence when viewed singly, they were in their *mean effects* strictly periodical phenomena, exhibiting by the character of their periodical variation a dependence on the sun as their primary source. To this important fact the disturbances of each of the three magnetic elements—the declination, the dip, and the intensity of the magnetic force—bore concurrent testimony, although the hours of maximum and minimum of their respective diurnal progressions were dissimilar, confirming the belief of distinct phenomenal laws in the disturbances of each of the elements.

The knowledge of the existence of periodicity in respect to the hours of the day was followed by the recognition of a periodical variation of a more remarkable kind, but which did not at first seem to connect itself with any other known phenomena, either cosmical or terrestrial. The employment of a constant quantity at each station, as a measure of disturbance at that station, had been adopted with the view of learning whether the aggregate amount of disturbance in different *years* was subject to any noteworthy variation. It soon appeared that this was the case. The observations made hourly at the colonial observatories, situated in very distant parts of the globe from each other, concurred in showing a progressive annual increase in the amount of disturbance from the year 1843 to the years 1848 and 1849, the two last being the years when the period for which the Government had sanctioned the hourly observations terminated. The increase was far too great to be ascribed to an accidental cause, the ratio of disturbance in 1848 and 1849 being more than double that in 1843 and 1844. The progressive increase was shown in each of the three elements, each having a distinct instrumental apparatus. The consistency of the evidence from so many sources seemed decisive as to the fact of a progressive increase during the years which have been named, wearing the appearance of being a portion of a periodical variation of which the cycle had yet to be discovered. Regret might well be felt that at this conjuncture the course of hourly observation, by which we had been thus far guided, had to be broken off, by the termination of the period for which the colonial observations had been sanctioned. Fortunately another indication was available which included a rather longer interval. It appeared that the increase of disturbances from 1843 to 1849 had been accompanied by a similar, though smaller, increase in the magnitude of the range of the diurnal oscillation of the three magnetic elements in those years, after due elimination had been made of the greater part at least of the effects of the disturbed observations. A less onerous system of observation, which had been commenced two years before and continued two years longer

than the full hourly system, sufficed to give the range of the diurnal oscillation for purposes of comparison; and the series thus afforded, extending from 1841 to 1851, showed that the diurnal range had been *greater* in 1841 and 1842 than in 1843 and 1844, from which date (concurrently with the aggregate amount of disturbance as already related) it had progressively increased to 1849, whilst 1850 showed no further advance, and 1851 a decided retrogression. Putting these facts together, there appeared sufficient ground on which to call the attention of those engaged in magnetic researches to the possibility of the existence of a periodical magnetic variation, which should have had its epoch of minimum about the years 1843 or 1844, and its epoch of maximum about the years 1848 or 1849; and to recommend the adoption of all available means of tracing its continuance in future years. In this view a full discussion of the particulars here adverted to was prepared for insertion in the Introduction to the second volume of the 'Hobarton Observations' then passing through the press, when, in the course of editing the English translation of the third volume of M. de Humboldt's 'Cosmos' from the proof sheets which were sent to me for that purpose by the author, my attention was arrested by the Table in p. 402 of that volume, containing the results of M. Schwabe's continuous and systematic observation of the solar spots from 1826 to 1850, accompanied by his own general conclusion to the following effect:—"The numbers in the Table leave no doubt that at least from 1826 to 1850 the solar spots have shown a period of about ten years, with maxima in 1828, 1837, and 1848, and minima in 1833 and 1843." Any hesitation that might have been felt in announcing the conjectured existence of such a periodical magnetic variation, unsupported by the coincidence of any other known cosmical or terrestrial variation of similar period, was at once removed. The variation observed in different years in the solar spots coincided perfectly with that which had just been recognized in magnetic phenomena which were otherwise connected with the sun by conformity to solar hours; and by this new and previously unsuspected relation, a very high degree of probability was given to the existence of a true and direct magnetic connexion between the earth and the central body of the solar system.

The facts, of which the general import has been thus hastily sketched, were made known to the Royal Society in March 1852, and were fully detailed in the Introduction to the second volume of the 'Hobarton Observations' published in the same month*. Of course great interest and curiosity attached to the question of

* The existence of "periodical laws discoverable in the mean effects of the larger magnetic disturbances" was first announced in a communication

how these phenomena would be found to comport themselves in succeeding years. Supposing the continuance of the same cycle in the variations both of the solar spots and of the magnetic disturbances which had been traced up to 1851, the next epoch of minimum might be looked for in 1853 or 1854. The British Colonial observatories having terminated, I availed myself, for the purpose of this investigation, of the means afforded by the hourly magnetic observations made by direction of the Russian Government at Pekin, from 1852 to 1855 inclusive. The observations themselves are published by M. Kupffer in the 'Annales de l'Observatoire central de Russie,' and are discussed by myself in the Introduction to the second volume of the 'St. Helena Observations,' published in 1860, pp. cxxxv. to cxliv. The ratios of disturbance in the years 1852 to 1855, taking the mean of the four years as the unit, were as follows:—

1852	1.01 to 1
1853	0.82 to 1
1854	0.79 to 1
1855	1.38 to 1,

marking the epoch of minimum as having occurred in 1853-54.

By the aid of the Kew Observatory (which owes its existence and support to the public spirit and liberality of the British Association, and which in 1857, by a grant from the Royal Society of a portion of the £1000 annually entrusted to their disposal by Her Majesty's Government, was enabled to complete an equipment of automatic instruments by which a continuous register of the variations of the three magnetic elements is maintained), we are able to trace the succeeding epoch of maximum: the aggregate magnetic disturbance was of nearly equal amount in 1858, 1859, and 1860; whilst in the following year (1861) a very considerable *decrease* showed that the maximum had passed and had taken place at the time anticipated, viz. in 1859, as the middle year of the three years of highest ratios. And here it may be desirable to remark that

presented to the Royal Society in January 1851, and printed in the Philosophical Transactions for 1851, Art. V.

The existence of a periodical variation in the magnetic disturbances, *coinciding in period and epoch with the variation in frequency and magnitude of the solar spots announced by M. Schwabe*, was communicated to the Royal Society (as stated in the text) in March 1852, and was printed in the Philosophical Transactions for 1852, Art. VIII.

The inequality or variation in the mean diurnal range, in different years and in different seasons, of the declination and horizontal force from 1841 to 1848 inclusive, and of the vertical force, inclination, and total force from 1842 to 1848 inclusive, was published in the same month as the last-named communication to the Royal Society, viz. March 1852, in vol. ii. of the 'Magnetical Observations at Hobarton in Van Diemen Island.'

the character of the variation both of the solar spots and of the magnetic disturbances is not so precisely conveyed as it might be, when certain single years are named as definite epochs of maximum and minimum. The character of the variation is, more approximately, that the years which end in 3, 4, and 5, are for the most part years of both minimum magnetic disturbance and minimum apparition of spots on the sun's disk. This last we may see in M. Schwabe's Table*, when we view together the columns indicating, on the one hand, the number of groups of spots seen in each year; and, on the other hand, the number of days in the year free from spots. In like manner the years ending in 8, 9, and 0 are for the most part distinguished by the nearly equal prevalence of the opposite extreme, both of magnetic disturbances and of the number of groups of spots, scarcely any day occurring in which the sun's disk is without spots. The intermediate years, viz. those ending with 1 and 2, and with 6 and 7, are usually years of medium in both respects—the two first generally exhibiting a diminishing, and the two last an increasing progression. The difference in the amount of magnetic disturbance in the years of maximum and minimum is approximately in the proportion of 5 to 2.

In our entire ignorance of the nature of the physical changes in the photosphere of the sun, of which the spots are a manifestation, we are not entitled to anticipate confidently that a variation which has shown itself with so much regularity since M. Schwabe's observations commenced in 1826, will continue to recur with the same regularity in subsequent years; nor ought we too confidently to infer that, even if the same general regularity be maintained, there may not be years in which apparent anoma-

*

Year.	Groups.	Days free from spots.	Days of Observation.	Year.	Groups.	Days free from spots.	Days of Observation.
1826.	118	22	277	1843.	34	149	312
1827.	161	2	273	1844.	52	111	321
1828.	225	0	282	1845.	114	29	332
1829.	199	0	244	1846.	157	1	314
1830.	190	1	217	1847.	257	0	276
1831.	149	3	239	1848.	330	0	278
1832.	84	49	270	1849.	238	0	485
1833.	33	139	267	1850.	186	2	308
1834.	51	120	273	1851.	151	0	308
1835.	173	18	244	1852.	125	1	337
1836.	272	0	200	1853.	91	4	299
1837.	333	0	168	1854.	67	65	334
1838.	282	0	202	1855.	38	146	315
1839.	162	0	205	1856.	34	193	321
1840.	152	3	263	1857.	98	52	324
1841.	102	15	283	1858.	188	0	335
1842.	68	64	307	1859.	205	0	343

lies, as we might term them, may present themselves. Should such take place in the phenomena of the solar spots, we have reason to expect that they may be accompanied by corresponding anomalies in the magnetic variations, if the causal connexion be indeed as close as our late experience would lead us to infer*. Meanwhile the conclusion which we may most safely draw is that it is highly desirable that both classes of phenomena should be carefully and independently observed and scrutinized.

In a discovery so unexpected and so recent as that of the cosmical connexion of the magnetic disturbances, it cannot be supposed that much can yet have become known to indicate the *mode* in which the sun's influence is exercised in the production of the phenomena of magnetic disturbance which we witness and record. The path in which we may most securely advance towards the attainment of this knowledge is by the careful and continuous study of the *effects* themselves, and by applying to them such processes of analysis as may appear most suitable to aid us in their comprehension. Already by such means we have advanced in a track which promises well, and seems deserving of being further pursued, and by a course more systematic than circumstances have hitherto permitted. The disturbances occurring at a particular station are resolvable, for example, into six distinct categories,—those which deflect the declination needle to the east, and those which deflect it to the west; those which increase and those which decrease the inclination or dip; and those which increase or which decrease the intensity of the magnetic force. This analysis has been made at a few stations; and the result fully confirms M. Gauss's anticipation that the phenomena of each of the different categories would be found to have distinct and independent laws. The research by which these are eliminated may indeed be tedious, requiring both time and labour, and the words of the great geometrician may frequently have to be borne in mind, "It will be a triumph of science should we at some future time succeed in arranging the manifold intricacies of the phenomena, in separating the individual forces of which they are the component result, and in assigning the source and the measure of each." That we are advancing in this course, and that the results which have already been obtained merit confidence as true representations of natural facts, may be exemplified by exhibiting in diagrams (Plate I. figs. 1

* The number of groups of spots observed in 1856 appears, from a notice from M. Schwabe in the *Astr. Nach.*, to have been less than might have been expected from the previous decennial progression shown in the Table from 1826 to 1850. The aggregate magnetic disturbance in the same year in the Nertchinsk series, 1851-57 (the only series that I have examined which includes 1856), exhibits a precisely similar anomaly.

and 2) the diurnal variation of the disturbances of the declination at stations widely remote from each other, separating the disturbances into their easterly and westerly constituents. I have selected for this purpose Kew in England, and Hobarton in Tasmania, as being stations divided from each other by nearly half the circumference of the globe. The eye can judge of the correspondence made known to us by the analysis which has been described. For the more ready comprehension of the Plate, the eye of the observer must be supposed to be at the centre of the magnet looking towards its north pole at Kew in the northern hemisphere, and towards its south pole at Hobarton in the southern hemisphere. The hours marked at the top and bottom of figs. 1 & 2 are those of local astronomical time at both stations. The curves represent the ratios of disturbance at the several hours, to the mean in the twenty-four hours taken as the unit. The ratios are read on the left side of the diagram. The unbroken curves are those at Kew, the broken those at Hobarton. The upper half of the diagram (fig. 1) represents westerly deflections, *i. e.* westerly as seen by the eye looking towards the north pole of the magnet at Kew, and towards its south pole at Hobarton. The lower half of the diagram (fig. 2) represents in like manner easterly deflections. It is scarcely possible when viewing the diagrams to doubt, on the one hand, that the easterly and the westerly deflections have distinct and independent laws, or, on the other hand, to resist the impression that the accordance of the allied phenomena in parts of the globe so remote from each other indicates community of origin and of laws*.

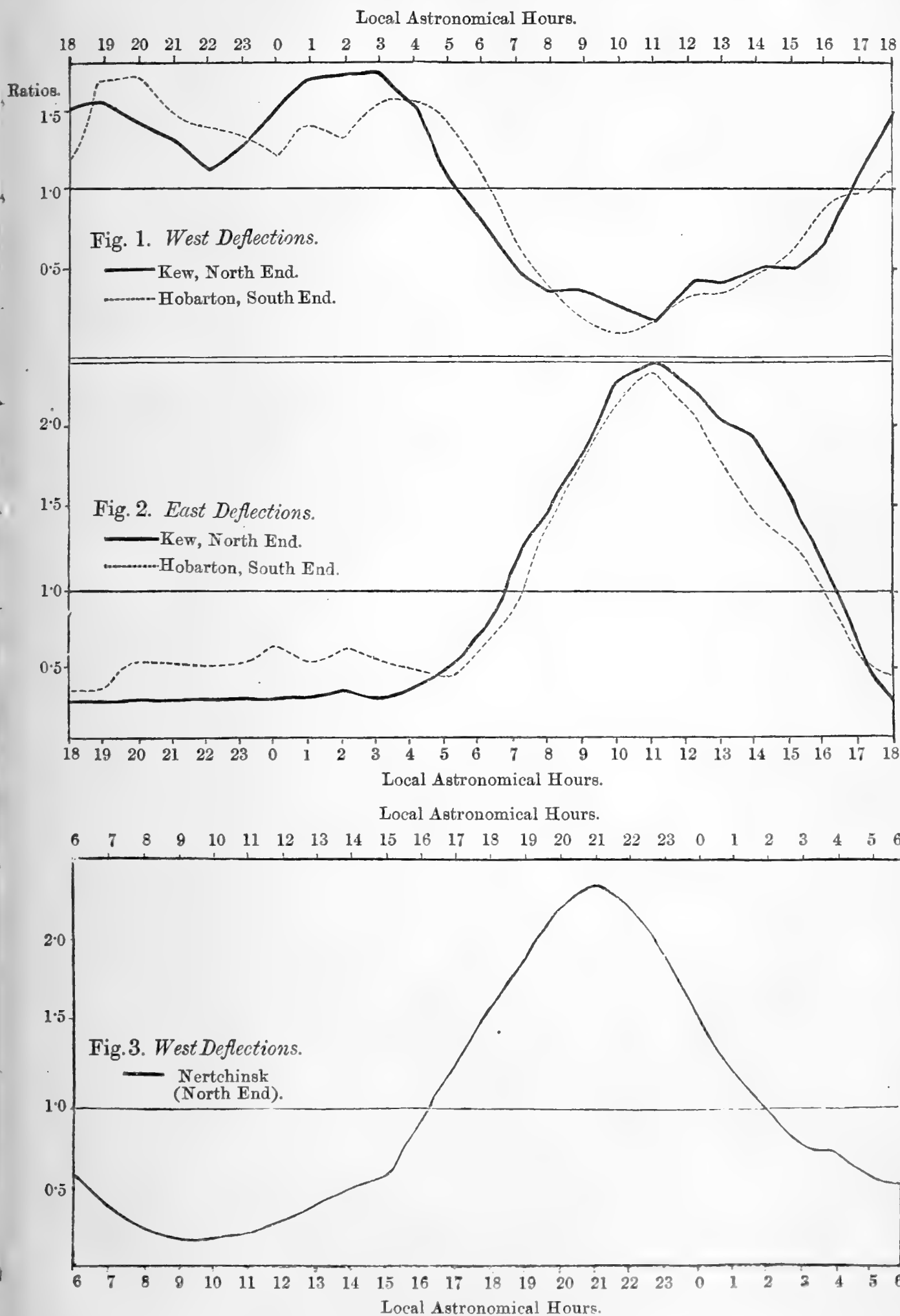
The ratios of the easterly and westerly deflections at the stations hitherto examined (amounting to several, and in both hemispheres), present generally diurnal variations conforming to one or other of the two types exhibited in the diagrams. So far there is a general agreement. But there are points of systematic difference at different stations which are no less noteworthy. These may perhaps be best explained, within the space which we can allot to them, by limiting our attention

* The deflections at Kew are derived from the hourly positions of the declination magnet tabulated from the Kew photograms in the four years 1858 to 1861 inclusive. Those at Hobarton from hourly observations continued during seven years and nine months, viz. from January 1, 1841, to September 30, 1848. The value taken as the indication of a disturbed position or observation has been a difference equalling or exceeding $3'3$ at Kew, and $2'13$ at Hobarton, from the respective normals of the same month and hour.

It happens that at Kew and Hobarton the hours of maximum easterly disturbance are nearly the same as the hours of minimum westerly disturbance; it may be proper, therefore, to remark that this is not a general rule; there are even stations where the easterly and westerly disturbance have their respective maxima nearly at the same hour.

Pl. I. *Mean Diurnal Disturbance-Variation: Declination.*

Figs. 1 and 2. Kew and Hobarton. Fig. 3. Nertchinsk.



to the form in fig. 2, which corresponds to the easterly deflections at Kew and Hobarton. During twelve hours, or about half the diurnal period, the amount of disturbance, and consequently the ratios, scarcely vary; there occurs then a sudden and rapidly progressive increase of disturbance continuing for about six hours, and forming an *apex* in the curve which at Kew and Hobarton occurs between eleven and twelve hours of local astronomical time; and from this apex there is a symmetrical and progressive decrease of disturbance lasting also about six hours. This is the general form, as I have stated, of *one* of the curves; but the local *hour* at which the apex occurs, although it happens to be the same at Kew and at Hobarton, appears to range through all the hours at different stations in both hemispheres. Another remarkable difference is found at different stations. The two forms exhibited in figs. 1 and 2 are so distinct and dissimilar, that there is no possibility of mistaking the one for the other. This dissimilarity is general between the two forms of the easterly and the westerly deflections; but the form exhibited in the Plate, as representing the *easterly* deflections at Kew and at Hobarton, does not always belong to the disturbances included in that category. In the northern hemisphere, for example, it belongs to the *easterly* deflections at Kew in England, and at Toronto and other stations in North America; but in Northern Asia, at Pekin, and at Nertchinsk in Siberia, it belongs, on the contrary, to the *westerly* deflections*. We have here, therefore, apparently a marked distinction in this respect between two parts of the same hemisphere; and there is another no less remarkable which may be conjoined with it, both appearing to point to geographical relations. At all the stations in North America, the easterly disturbances predominate in amount, whilst in Northern Asia it is the westerly which predominate†. There are therefore distinguishing characters in the effects of the sun's influence

* Fig. 3 in Plate I. represents the ratios at the several hours of the *westerly* disturbances at Nertchinsk in Siberia, lat. $51^{\circ} 56' N.$, long. $116^{\circ} 33' E.$ from Greenwich, derived from hourly observations during the years 1851 to 1857 inclusive. The difference from the normal constituting a disturbed observation has been taken at $1''.96$. The form of this curve has the same general character as the curve of *easterly* deflection at Kew (fig. 2) and differs altogether from the form of the *westerly* curve in fig. 1.

The curve of the easterly deflections at St. Helena (south end of the magnet) is drawn in the woodcut in vol. xi. p. 683, of the 'Proceedings of the Royal Society,' and is another repetition of the same general form.

† A similar difference is found in the preponderance of easterly or westerly deflections in the southern hemisphere. At Hobarton, the Cape of Good Hope, and St. Helena, easterly deflections (of the south end of the magnet) preponderate; at the Falkland Islands, on the other hand, it is the westerly deflections (of the same end of the magnet) which preponderate.

in producing the disturbances in different parts of the globe which have a systematic aspect, and are not unlikely to lead to important generalizations. What has been here stated in respect to the declination appears also to be the case in the other two magnetic elements, though requiring more labour and patience in the deduction.

There is yet one other and most notable distinction with reference to geographical relations: the aggregate amount of disturbance varies greatly and instructively in different parts of the globe; it is small in the intertropical regions, and augments in the middle latitudes, but by no means in a ratio dependent on the increase of latitude; for as the higher latitudes are approached, the disproportion of disturbance in different *meridians* becomes excessive, and leads to the inference that in both hemispheres there are localities indicated by the peculiar magnitude of the disturbing influence as being those where that influence may possibly enter on the terrestrial surface, and from whence it may be propagated with progressively decreasing intensity. Such a locality is found in the north-western part of the American Continent. Hourly observations made during seventeen months in 1852, 1853, and 1854, at Point Barrow, show an aggregate amount of disturbance unparalleled elsewhere, so far as our knowledge extends—*greatly* exceeding the amount of disturbance at other stations of hourly observation in the Canadian and Hudson's Bay territories or in high arctic latitudes, which amount is itself much greater than is found, or than may be inferred to exist, in corresponding latitudes on this side of the Atlantic.

Point Barrow is no less distinguished for the extraordinary prevalence of Aurora, which is now known to be a concomitant phenomenon with magnetic disturbances. During six winter months, three in 1852-53, and three in 1853-54, Aurora was present and visible at nearly one-third of the times of hourly observation, a proportion far beyond any that has been recorded elsewhere, marking, in common with the excessive amount of magnetic disturbance, the striking magnetic peculiarity of the locality.

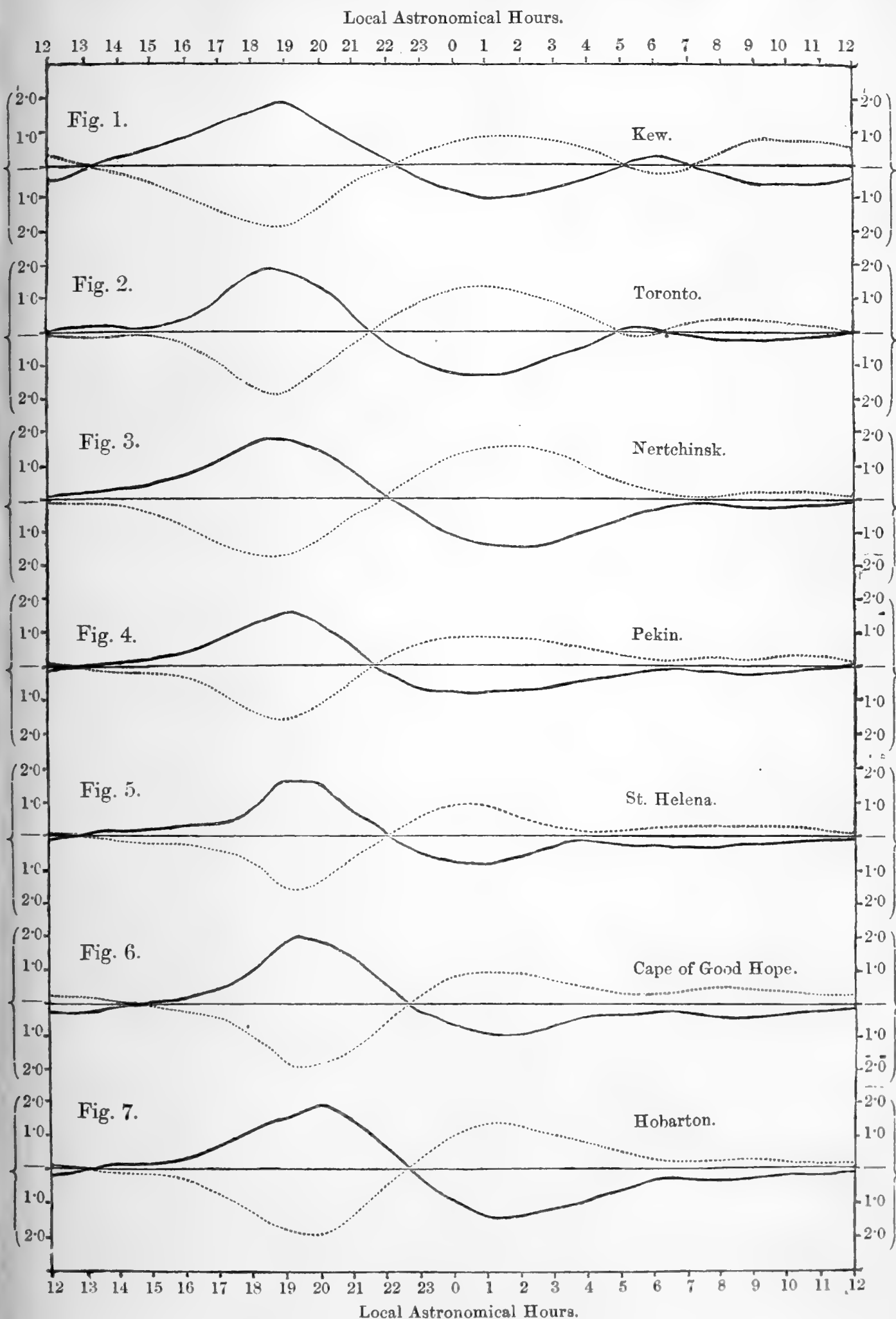
II. The next cosmical feature which I desire to bring under your notice has been made known to us by discovering the existence of a "semiannual inequality" in the diurnal variation of the declination. When hourly observations have been made at any station on the globe, and the magnetic disturbances (or at least the greater portion of them) have been eliminated by the process already referred to, the hourly positions of the magnet (*i. e.* its mean position at each of the twenty-four hours) may be collected into monthly Tables, or Tables corre-

sponding to each month, showing the mean or normal positions at each hour. When these monthly Tables are compared, the results are found to arrange themselves into two distinct categories, the one comprising the six months in which the sun is in the northern signs of the zodiac, and the other the six months in which he is in the southern signs. The difference of the diurnal variation shown by the two categories is of a very marked character, and is always the same in whatever part of the globe the station of observation is situated. It is obviously a periodical affection common to the whole globe, having a year as its cycle, with semiannual epochs coinciding as nearly as may be with the equinoxes. A semiannual variation of similar character is shown in each of the three magnetic elements; but, as the facts of the declination have been more extensively examined than those of the two other elements, I have confined myself to them in Plate II. which illustrates this particular case of the solar action*. In this Plate the several hours of local astronomical time at each of the stations are shown by the figures at the top and bottom. The unbroken curves in each figure correspond to the months from April to September, when the sun is in the northern signs; and the broken curves to the months from October to March, when he is in the southern signs. The divergence of the unbroken and broken curves respectively from the mean annual solar-diurnal variation at the station (which for this purpose is represented in each of the figures by a horizontal line) shows the characters of the semiannual irregularity in the two opposite categories. The Plate includes stations in each

* Plate II. Semiannual inequality:—

- Fig. 1. At Kew, from hourly positions of the declinometer magnet from June 1, 1858, to December 31, 1861, inclusive; omitting disturbed positions equalling or exceeding a difference of $3'3$ from the normal of the month and hour.
- Fig. 2. At Toronto, from hourly observations from 1843 to 1848 inclusive; omitting disturbed observations equalling or exceeding a difference of $3'6$.
- Fig. 3. At Nertchinsk, from hourly observations from 1851 to 1857 inclusive; omitting disturbed observations equalling or exceeding a difference of $1'96$.
- Fig. 4. At Pekin, from hourly observations from January 1, 1852, to October 31, 1855, inclusive; omitting disturbed observations equalling or exceeding a difference of $2'25$.
- Fig. 5. At St. Helena, from hourly observations from September 1, 1842, to August 31, 1847, inclusive; omitting disturbed observations equalling or exceeding a difference of $1'78$.
- Fig. 6. At the Cape of Good Hope, from hourly observations from October 1, 1841, to September 30, 1846, inclusive; omitting disturbed observations equalling or exceeding a difference of $1'88$.
- Fig. 7. At Hobarton, from hourly observations from January 1, 1841, to September 30, 1848, inclusive; omitting disturbed observations equalling or exceeding a difference of $2'13$.

Pl. II. *Semiannual Inequality of the Solar-diurnal Variation.—Semiannual Means, April to September —; Ditto, October to March.....*



of the four quarters of the globe, in the tropics as well as in the middle latitudes, and in both hemispheres; and it will be seen that the divergence of either the unbroken or the broken curve at any particular hour is substantially the same in all the figures.

There is another kind of semiannual inequality, also therefore attributable to solar influence, which I am unwilling to pass without notice even in this brief review, as it has been satisfactorily traced, admits of simple representation, and is perhaps particularly likely to be suggestive to some among my hearers. In this case I have to speak of the two other magnetic elements, the intensity of the magnetic force, and the dip (or inclination below the horizontal line) of the magnetic needle when free to move in the vertical plane; and I am not here concerned either with their variations in the diurnal period, or with the transitory disturbances which may affect them. Both the dip of the needle and the intensity of the force acting upon it can be determined with much precision when proper instruments are employed, and the observers are well trained. Such determinations, made on stated days in each month, have been continued for several years at some stations, without any change either of instruments or of observers; and it has been found that in the high or middle latitudes of both hemispheres there occurs a small increase both in the dip and in the force in the months when the sun is nearest to the earth as compared with the months when his distance from our planet is greatest, *i. e.* that both the dip and the total force are greater in both hemispheres in December than they are in June. This effect cannot, of course, be ascribed to any influence of the seasons of summer and winter affecting either the earth or the needle by their different temperatures; for the magnetic difference is the *same* in both hemispheres, while the *seasons* are *opposite*, the higher force and dip of December being associated with winter in our hemisphere and with summer in the southern hemisphere. The amount of the difference, though small in itself, is large when tested by the amount of probable error. It was first shown by the concurrent testimony of several years of observation at Toronto in Canada, and Hobarton in Tasmania. The observations of the five years since the Kew Observatory has been in action have furnished for the northern hemisphere a full corroboration of the inference from the results at Toronto. It would be very desirable to have a similar corroboration of the results at Hobarton, by equally careful observations continued for a few years at some other station in a similar latitude in the southern hemisphere.

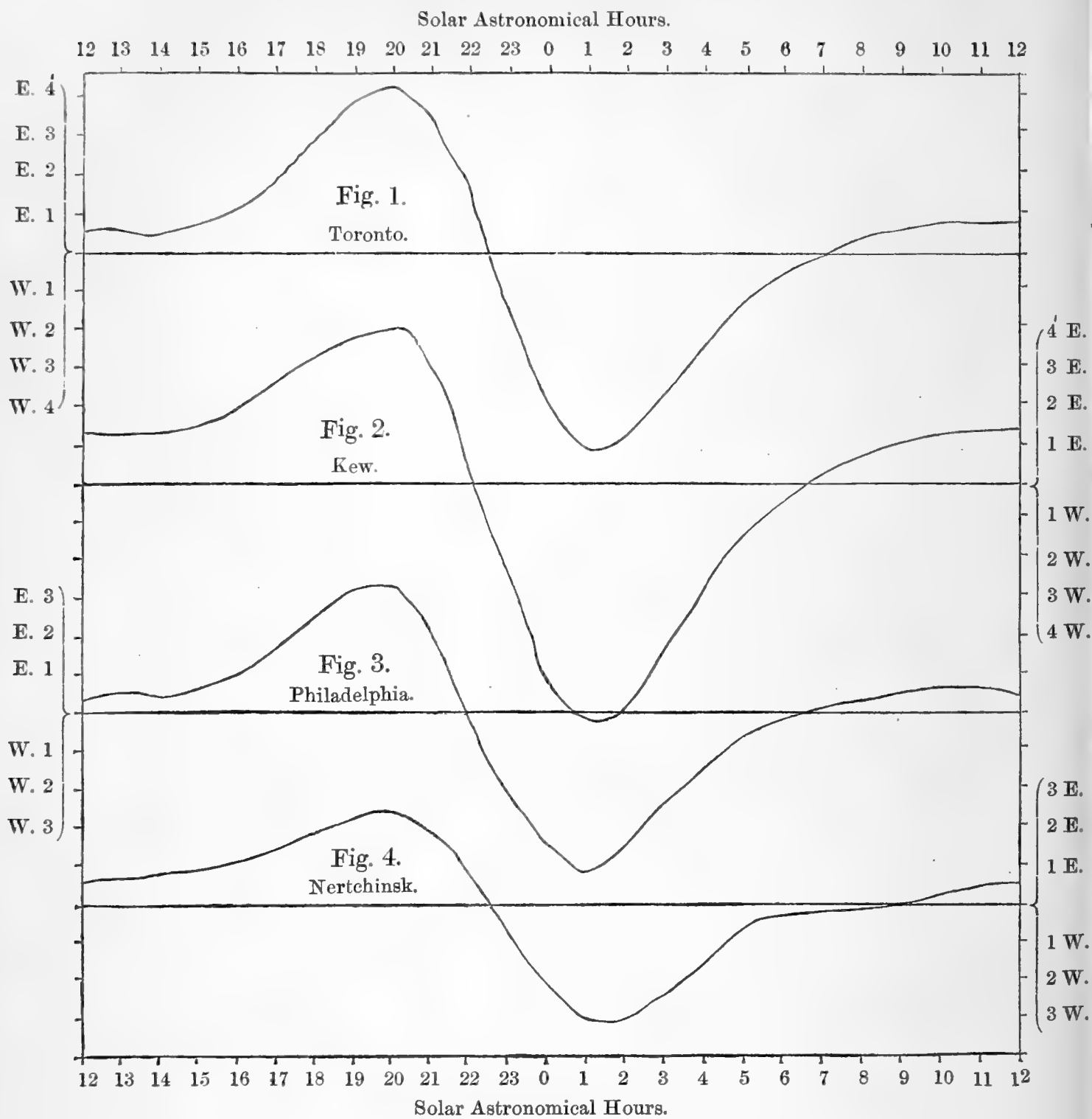
III. When a mean of the undisturbed observations at each hour is taken for the entire year, the semiannual inequality of

which we have recently spoken merges into an annual *mean solar diurnal variation*, evidencing its cosmical character and its connexion with the sun, first by its period being a solar day, and secondly by its amount in different years being subject to a variation which corresponds in period and epochs with the decennial variation of the solar spots. In both these respects its characteristics are the same as those of the disturbance diurnal variation; and thus a community of origin is indicated. But it has other features which constitute a well-marked distinction between the two classes of phenomena; and thus we have also reason to infer that, together with community of origin, there is a difference in the *mode of action* by which each is produced. When, by the elimination of the larger portion of the disturbances, we have obtained in the residual values a satisfactory approximation to the true solar-diurnal variation, we find that the general form of the curve so obtained is substantially the same at all stations in the middle latitudes of the same hemisphere. In the northern magnetic hemisphere it has an eastern extreme about 8 A.M., and a western extreme between 1 and 2 P.M., returning to its mean position about 7 P.M. During the hours of the night the needle is comparatively tranquil. In Plate III. figs. 1, 2, 3, 4, the curves of the mean solar-diurnal variation are represented for Toronto, Kew, Philadelphia, Nertchinsk, all stations in the northern hemisphere, but varying greatly in respect to the circumstances of land and sea, near or remote. In having but one easterly and one westerly extreme, the solar-diurnal variation resembles one of the forms of the disturbance-variation, viz. that figured in the first Plate as representing the easterly disturbances at Kew and Hobarton; but there is this essential distinction between them, that in the disturbance-variation the local solar hour belonging to any point in the curve—the apex, for example, as the most marked feature—varies apparently without limit in different meridians, whilst in the solar-diurnal variation, the *time* as well as the form is constant in the extra-tropical latitudes of all the meridians of the same hemisphere*.

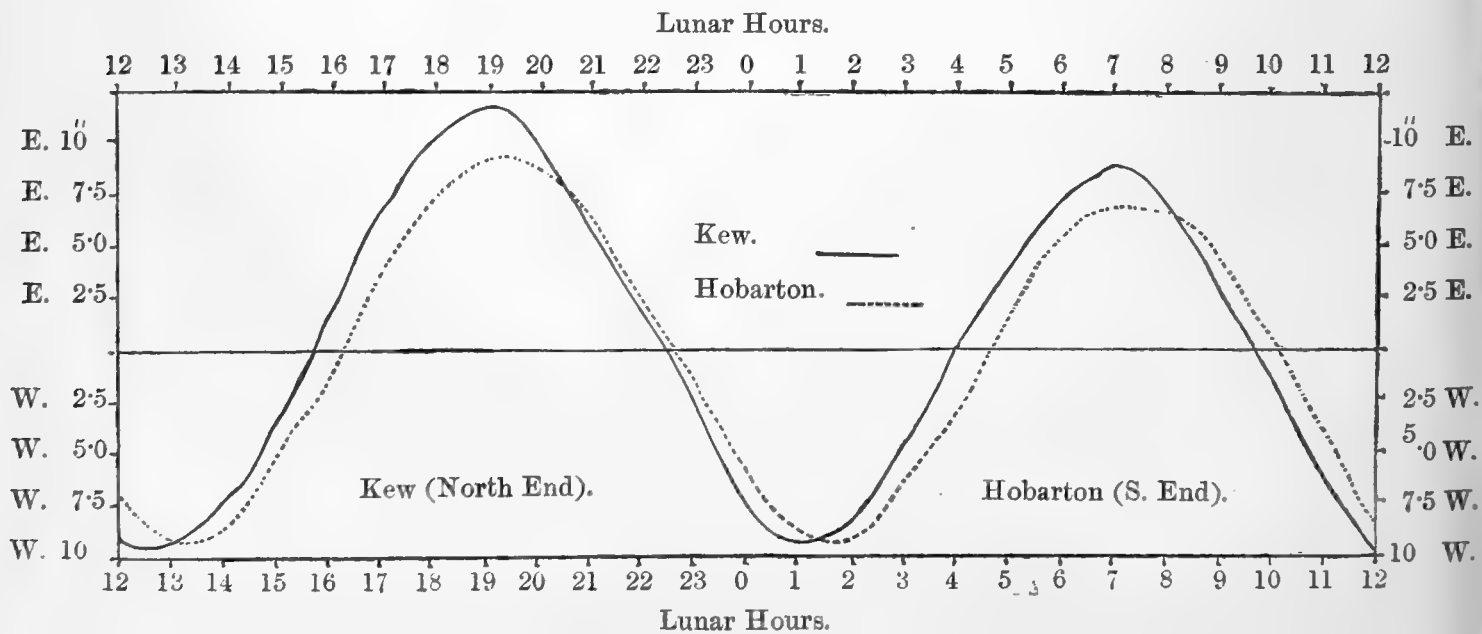
* The easterly and westerly deflections spoken of in the text are *magnetic* east and west, which are not necessarily nor always the same as geographical east and west. In the lands visited by the arctic expeditions, situated to the north of the point where the dip of the needle is 90° , the declination is 180° , or, in other words, the marked or north end of the magnet is directed towards the geographical *south*. In such localities the deflections towards the magnetic east are in fact deflections towards the geographical west, and *vice versa*. At 8 A.M., for example, the north end of the magnet is deflected towards the *magnetical east*, or, otherwise expressed, towards the *geographical west*; and at 2 P.M. towards the magnetic west, or geographical east. But in the same meridian, and in localities situated on the opposite or south side of the point of 90° of dip, the marked or

Phil. Mag. S. 4. Vol. 24. No. 159. Aug. 1862. I

Pl. III. *Solar-diurnal Variation: Declination.*



Lunar-diurnal Variation: Declination.



There is a second very notable geographical distinction between the phenomena of the solar-diurnal and of the disturbance-variations, and it is one which bears strongly on the probability of a difference in the mode of the sun's action in producing the two classes of effects. It consists in the very different relative magnitudes of the range of the two variations in different parts of the same hemisphere. In the case of the disturbance-variation, we have already noticed that its amount is much less in the middle latitudes of Europe and Asia than in Canada, while in certain parts of the north-western portions of the American Continent it attains a development which renders the disproportion excessive. We have nothing analogous to this in the solar-diurnal variation, which exhibits in different meridians a remarkable approach to constancy in its *amount* (varied only by distance from the dividing line between the two magnetic hemispheres, and by differences in the antagonistic horizontal force of the earth), as well as in its form and turning-hours—a constancy which contrasts strongly and systematically with the phenomena of the disturbance-variation, and is apparently quite uninfluenced by any peculiarities of land and sea, near or remote.

I have yet another remarkable distinction to bring before you, which seems to indicate a difference in the mode of operation in the production by the sun of the mean solar-diurnal variation of the entire year, and of the semiannual inequality previously described to you. In the latter, if you will look again at Plate II., in which it is represented in various latitudes and in both hemispheres, you will see everywhere a striking uniformity; the amount is nearly the same at all the stations, and the same end of the needle is deflected in the same direction at the same local hours in both hemispheres. In the mean annual solar-diurnal variation, on the other hand, the same end of the needle is deflected in opposite directions at the same local hour in the two hemispheres; the variation itself disappears altogether as the line of separation between the two magnetic hemispheres is

north end of the magnet being directed towards the north, the solar-diurnal variation is at 8 A.M. to the east, and at 2 P.M. to the west, both magnetically and geographically. The arctic voyages and travels have shown that between these extreme cases of dissimilarity every possible intermediate variety may be found. Thus, whilst the deflection caused by the solar-diurnal variation is always in the same *magnetic* direction at the same local solar hour, its geographical direction over a very considerable area around the dip of 90° has every conceivable variety. We must regard, therefore, the causal action which produces the solar-diurnal variation as operating through the intermediation of the earth's magnetism, and not simply as a direct action upon the needle itself. In these remarks on the solar-diurnal variation, it is always to be understood that the effects of the disturbances have been either wholly or for the most part eliminated.

approached, and progressively increases as that line is receded from on either side.

IV. We have next to notice a magnetic variation which has for its period a *lunar* day, thereby establishing the fact of the existence of a sensible magnetic influence exercised by the *moon* at the surface of the earth. It is derived in the following manner:—Hourly observations having been made at the several hours of mean solar time, and having been subjected to processes by which the effects of the magnetic disturbances and of the solar diurnal variation have been wholly or for the most part eliminated, the residual values are rewritten in lunar monthly Tables, entering each value in the column of the lunar hour nearest to the actual time of observation. The monthly means of the several *lunar* hours then show the mean lunar-diurnal variation in that lunation. This has been done for the three magnetic elements at stations situated in both hemispheres and in the tropics, as well as in the higher latitudes. The result has been to give everywhere and in each of the three magnetic elements the same highly characteristic general form for the curve of the lunar-diurnal variation, that form being essentially and systematically different from that of the solar-diurnal variation. For the sake of brevity, I will confine myself, as before, to the phenomena of the declination. In so doing, we find *four* culminations, two of maximum easterly, and two of maximum westerly deflection, all nearly equal in magnitude, and separated by nearly equal intervals of lunar time. Thus at Kew, for example, we obtain from the mean of four years of observation (1858 to 1861 inclusive) the curve represented by the unbroken line in Pl. III., where it will be seen that we have at the lunar hours of 1 and 13 extreme westerly deflections amounting to about $10''$ from the zero line, and at the lunar hours of 7 and 19 extreme easterly deflections of nearly the same magnitude, the zero line being passed through four times in the twenty-four hours at nearly equal intervals. I have figured in the same Plate and figure, by a broken line, the curve of the corresponding phenomena at Hobarton; because, without entering into particulars, which would require more time than can be given, the juxtaposition of the two curves illustrates both the correspondence and the differences in the lunar-diurnal variation in different parts of the globe. The correspondence shows itself to the eye by the general agreement in the form of the curves. The principal difference consists in the circumstance that, Kew being in the northern magnetic hemisphere, the deflections represented are those of the north end of the magnet; and Hobarton being in the southern magnetic hemisphere, the deflections represented are those of the south end of the magnet. It will be seen that the deflec-

tions in the two curves are in the same direction at nearly the same hour; but the deflections represented are at Hobarton those of the south end of the magnet, and at Kew those of the north end of the magnet. No decennial influence is perceptible in the lunar-diurnal variation.

The peculiar feature of the lunar-diurnal variation, which is consistently manifested in all the elements and at all the stations, is that of a double and symmetrical progression in each lunar day. This has been regarded by some physicists as indicating that the magnetism of the moon, of which it is the effect, is an *induced* magnetism. There are, however, difficulties in this supposition which appear to stand in the way of its immediate and general reception. Meanwhile every additional locality in which the phenomena are made the subject of satisfactory investigation enlarges the basis of induction on which the physical explanation of the magnetic relations undoubtedly subsisting between the earth and her satellite must eventually rest*.

V. I have thus brought before you some of the most notable instances in which a cosmical connexion has been traced between the sun and moon and magnetic variations observed on the earth. They are all furnished by phenomena which, in the Report of the Royal Society, were classified as being either periodical or irregular, but which may now be comprised under the single name of "periodical variations," inasmuch as those which were supposed to be "irregular" are now known to be subject in their occurrence to laws of regular periodical action. But there still remains a distinct class of magnetic variations which there is also much reason to believe are, on a more extended scale, periodical, and probably governed by cosmical relations, to the discovery of which, however, we as yet possess no clue. Many of my hearers are familiar with, and others may have heard of, the so-called "isogonic," "isoclinal," and "isodynamic" magnetic lines, which, traced on maps of the globe or of any particular portion of it, represent the results of "magnetic surveys," and show the distribution of the magnetic influence on the surface of our planet. When any such map, based on observations of a given year for example, is compared with one based on observations of an earlier or later epoch, changes are seen to have taken place which we believe to be in accordance with laws of regular periodicity extending over a cycle of several centuries, though

* The second volume of the 'St. Helena Magnetic Observations,' published in 1860, contains the lunar-diurnal variations of the declination, inclination, and total force at Toronto, St. Helena, and the Cape of Good Hope; of the declination and inclination at Hobarton; and of the declination at Kew and Pekin. The lunar-diurnal variation of the declination at Nertchinsk, and of the total force at Hobarton, are prepared for publication.

any more particular statement regarding its duration could be as yet little more than conjectural. These variations, therefore, still retain the name at first assigned to them of *secular* changes. That they are eminently systematic,—that in all parts of the globe they manifest themselves as due to a common cause,—and that this cause is not traceable to any terrestrial changes with which we are acquainted or can reasonably conceive,—are considerations which confer a very high interest on the investigations connected with this branch of magnetical science. We owe the first clear conception of the distribution of terrestrial magnetism, and of the systematic character of the changes taking place in it from epoch to epoch, to two papers communicated to the Royal Society towards the close of the seventeenth century by Dr. Halley, followed in 1701 by his general map of the isogonic lines, or lines of equal declination, which were thence long called Halleyan lines*. Conducting his investigations in the true spirit of inductive inquiry, he formed by careful research, and gave in his first paper in 1683, an extensive Table of the most trustworthy observations of the declination (the only magnetic element then obtainable) in different parts of the globe, and by the study of these he arrived at the conclusion that they admitted of explanation upon the hypothesis of four magnetical poles or points of attraction, two in each hemisphere, situated not far from the geographical poles,—the needle in those parts of the earth which lie adjacent to any one of these magnetic poles being governed thereby, the nearest pole being predominant over the more remote, whilst in the equatorial regions the magnetic direction became more complex, inasmuch as the influence of the four poles required to be taken into account. He obtained by his hypothesis a representation of the phenomena so satisfactory, that he expressed the conviction (using his own words) that “he had put it past doubt that there are in the earth four such magnetical points or poles which occasion the great variety and seeming irregularity which is observed in the variations of the compass;” the paper contained the geographical coordinates of the positions assigned by him to his four poles for the epoch at which he wrote. In his second paper, in 1692, Dr. Halley discussed at large the phenomena of the *secular changes* of the declination, showing that, as far as they were then known, they might be explained by regarding two of the poles as fixed or stationary, and the two others as subject to a gradual and progressive movement of translation—the northern to the east, and

* This was the first introduction of a mode of representing in a combined view a large body of facts collected from different sources and referring to different localities, which has been since found of great value in many branches of natural science.

the southern to the west—in a periodic revolution, of which the cycle might perhaps be roughly estimated at 700 years*.

Since the time of Halley, several maps of the declination or isogonic lines have been published from time to time from the observations of successive epochs. There have also been published maps of the other two magnetic elements; and these, which embody facts unknown at the date when Halley wrote, have confirmed in a remarkable degree his inference, drawn from the declination only, of the existence of four poles or points of principal magnetic attraction. In the higher latitudes of both hemispheres the isoclinal and isodynamic lines have the form of lemniscates, each loop of which encloses and surrounds a point of maximum force. In each one of the three elements the maps corresponding to successive epochs indicate a secular change in remarkable accordance, both in character and degree, with the views entertained by Halley. When carefully studied, the changes in all parts of the globe are seen to concur in indicating a gradual and progressive translation of his two moving poles in the directions assigned by him. The gradual and extremely regular character of this movement has recently been strikingly illustrated by the results of observations, conducted with suitable care and under particularly favourable circumstances, which enable us to trace the progress of the secular change, not merely from year to year, but actually from week to week, each week showing an equal aliquot part of the yearly change. Whether we regard the connected generality of the phenomena, or their minute regularity, it appears difficult to resist the impression that we see in them the indication of some yet unknown cosmical relation. Be this, however, as it may, the path of inductive research lies clearly before us: it is our part to advance in it by continuing diligently and perseveringly the investigation of the facts which are accessible to us on our own globe, comprising (should the supposed periodicity be confirmed) the duration of its cycle and the characters of its epochs. The accomplishment of these objects is being sought by what are termed *magnetic surveys*, repeated after the lapse of a suitable number of years, and disclosing by their comparison the changes that have taken place in the areas over which they extend. Such a survey was made in our own country in 1837, at the instigation of the British Association, and was speedily followed by similar undertakings in the Austrian and Bavarian states, in the British possessions in North America, in the Indian Archipelago, and more recently in British India. Amongst the most important, and certainly the most arduous of these labours, must be reckoned the determinations of the mag-

* In this hypothesis the two fixed poles may be considered to be those proper to the earth itself as a magnetic body, and the two moving poles to be those of a superinduced magnetism.

netic lines in the arctic and antarctic regions,—foremost in difficulty, indeed, but also foremost in value. Time will not allow me to specify the numerous and valuable data obtained by our naval and military officers, or by our travellers, or by those of other nations. The coordination of this large mass of materials is necessarily a work of time; but much has already been accomplished*, and the work is advancing towards completion. We shall by these means possess a general representation of the magnetic state of the globe corresponding to a particular epoch (1840) based wholly on observations made within a few years of each other, and admitting of easy reduction to a common epoch. A repetition of such a work, after a sufficient interval shall have elapsed, will give a second representation, comparable with the first, and furnishing by that comparison far more extensive and accurate data than we have hitherto possessed for the secular changes in all parts of the globe. Already the survey of Scotland and England, executed for the year 1837, has been repeated for the year 1860; and the result has shown the sufficiency of an interval of even less than a quarter of a century to determine with great precision the changes which have taken place in the values and directions of the magnetic lines, making it clear that perseverance in the systematic mode of investigation which has been commenced will put us ultimately in possession of the laws of the phenomena of our own globe.

It *may* be that our knowledge may have its limit there; and that the cosmical or other physical causes in which the secular change originates may not be discernible. Without the discovery of the decennial variation of the solar spots, the cosmical relation of the magnetic variations having the same period would have remained unknown to us; and it is quite conceivable that cosmical relations may exist in the case of the secular changes which may not reveal themselves to us by any external manifestations.

We cannot perhaps close this brief and imperfect notice of the interest attaching to the secular magnetic variations more appropriately, than by recalling the remarkable words with which Dr. Halley terminated his own discussion of the subject in his first paper, written nearly two centuries ago:—"Whether the magnetical poles move with one motion or with several,—whether equally or unequally,—whether circular or libratory,—if circular, about what centre,—if libratory, after what manner,—are secrets as yet unknown to mankind, and are reserved for the industry of future ages."

* "Contributions to Terrestrial Magnetism," Nos. I. to IX. (Phil. Trans. 1840, Art. IV.; 1841, Art. II.; 1842, Art. II.; 1843, Art. IX.; 1843, Art. X.; 1844, Art. VII.; 1846, Arts. XVII. and XVIII.; 1849, Art. XII.).

XV. *Note on the History of the Dynamical Theory of Heat.*
By J. P. JOULE, LL.D., F.R.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

WILL you permit me to trouble your readers with a few remarks on the subject of my friend Professor Tyndall's lecture at the Royal Institution, reported in your last Number? In this lecture he enforces the claims of M. Mayer, a philosopher whose merit has perhaps been overlooked by some of our English physicists, and unaccountably so by his fellow-countrymen. I myself was only imperfectly acquainted with his papers when, in good conscience and with the materials at command, I gave a sketch of the history of the dynamical theory of heat, in my paper published in the Philosophical Transactions for 1850. M. Mayer's merit consists in having announced, apparently without knowledge of what had been done before, the true theory of heat. This is no small merit, and I am the last person who would wish to detract from it. But to give to Mayer, or indeed to any single individual, the undivided praise of propounding the dynamical theory of heat, is manifestly unjust to the numerous contributors to that great step in physical science. Two centuries ago, Locke said that "Heat is a very brisk agitation of the insensible parts of the object, which produces in us that sensation from whence we denominate the object hot; so that what in our sensation is *heat*, in the object is nothing but *motion*." In 1798, Rumford, inquiring into the source of heat developed in the boring of cannon, observed that it was "extremely difficult, if not quite impossible, to form any distinct idea of anything capable of being excited and communicated, in the manner the heat was excited and communicated in these experiments, except it be motion." In 1812, Davy wrote, "The immediate cause of the phenomena of heat, then, is motion, and the laws of its communication are precisely the same as the laws of the communication of motion"*; and he confirmed his views by that original and most interesting experiment in which he melted ice by friction. In 1839, Séguin published a work entitled *De l'Influence des Chemins de Fer*. He shows that the theory generally adopted would lead to the absurd conclusion that a finite quantity of heat can produce an indefinite quantity of mechanical action, and remarks (p. 328), "Il me paraît plus naturel de supposer qu'une certaine quantité de calorique disparaît dans l'acte même de la production de la force ou puissance mécanique, et réciproquement." At p. 383 he remarks, "La force mécanique qui apparaît pendant l'abaissement de température d'un gaz comme de tout autre corps qui se dilate, est la

* Elements of Chemical Philosophy, p. 94.

mesure et la représentation de cette diminution de chaleur.” In p. 389 he gives a Table of the quantity of mechanical effect produced corresponding to the loss of temperature of steam on expanding. From this it appears that 1° Cent. corresponds with 363 kilogrammes raised to the height of 1 metre. At p. 403 he states, “Je bornerai là mes réflexions sur un sujet dont chacun saura apprécier l’importance. Du calorique qui est employé par l’industrie à produire de la force, et aux usages domestiques, une faible partie seulement est utilisée ; une autre quantité bien plus considérable, et qui pourrait suffire à créer d’immenses valeurs et à augmenter d’autant la richesse nationale, se trouve absolument perdue.” From the above extracts, it will be seen that a great advance had been made before Mayer wrote his paper in 1842. Mayer discourses to the same effect as Séguin, but at greater length, with greater perspicuity, and with more copiousness of illustration. He adopts the same hypothesis as the latter philosopher, viz. that the heat evolved on compressing an elastic fluid is exactly the equivalent of the compressing force, and thus arrives at the same equivalent, viz. 365 kilogrammes per 1° Cent.

It must be remarked that, at the time Séguin and Mayer wrote, there were no known facts to warrant the hypothesis they adopted. There was no reason to assert that the heat evolved by compressing a gas was even approximately the equivalent of the compressing force. This being the case may account for the inattention of the scientific world to these writings. The dynamical theory of heat certainly was not established by Séguin and Mayer. To do this required experiment ; and I therefore fearlessly assert my right to the position which has been generally accorded to me by my fellow physicists as having been the first to give a decisive proof of the correctness of this theory.

In saying this I do not wish to claim any monopoly of merit. Even if Rumford, Mayer, and Séguin had not produced their works, justice would still compel me to share with Thomson, Rankine, Helmholtz, Holtzman, Clausius, and others, whose labours have not only given developments and applications of the dynamical theory which entitle them to merit as well as their predecessors in these inquiries, but who have contributed most essentially in supporting it by new proofs.

Permit me to remark, in conclusion, that I applied the dynamical theory to vital processes in 1843* ; and that in 1847, in a popular lecture published in the ‘Manchester Courier,’ I explained the phenomena of shooting-stars, and also stated that the effect of the earth falling into the sun would be to increase the temperature of that luminary†. Since that time Thomson,

* Phil. Mag. S. 3. vol. xxiii. p. 442.

† Ibid. vol. xxxii. p. 350 ; and Manchester Courier, May 12, 1847.

by his profound investigations, has made the dynamical theory of heat, as applied to cosmical phenomena, his own.

I sincerely trust that, by the foregoing remarks, I have done no injustice to Mayer, especially as I grieve to hear that sickness has removed him (I hope for only a short time) from the science to which he has contributed with so much ability. The reproduction of some of his papers in the *Philosophical Magazine*, particularly that "On the Forces of Inorganic Nature," would, I am sure, interest many of your readers, and enable them to fully appreciate his just claims.

I remain, Gentlemen,

Yours respectfully,

J. P. JOULE.

XVI. *On some of the Basic Salts of Copper.* By FREDERICK FIELD, F.R.S.E., Professor of Chemistry at the London Institution*.

BASIC Sulphate of Copper.—The analyses of this compound, made from time to time by chemists, are not very concordant, the oxide of copper varying in some instances as much as 3 per cent. in different estimations, causing the formula of the salt to range between $4\text{CuO}, \text{SO}^3, 4\text{HO}$, and $4\text{CuO}, \text{SO}^3, 3\text{HO}$.

These discrepancies may partly be accounted for by the different modes adopted for the preparation of the substance; as sometimes it is formed by the digestion of the hydrated oxide, or basic carbonate, in a solution of the monosulphate, and at others by the imperfect precipitation of the latter salt by the caustic alkalies. Graham observes that, when formed by precipitation, it is impossible to remove the last trace of sulphate of potash, even by prolonged washing, this salt being carried down with the precipitate.

The mineral Brochantite has also been examined with much care; and although the results differ slightly, it may certainly be regarded as quadribasic, associated with four atoms of water. A very pure sample, containing no earthy admixture or any trace of carbonate, from Andacollo, Chile, differs but little from a specimen from Mexico, analysed by Berthier.

	Mexico (Berthier).	Chile (Field).
Oxide of copper . . .	66·20	66·94
Sulphuric acid . . .	16·60	16·59
Water	17·20	16·47
	<u>100·00</u>	<u>100·00</u>

The specific gravity of the specimen from Chile was 3·81.

* Communicated by the Author.

No difficulty was experienced in obtaining the artificial salt of definite composition, by the addition of a small quantity of caustic potash to a great excess of sulphate of copper, and boiling for some time. On filtering off the excess of sulphate, and washing for many days until no trace of sulphuric acid or copper could be detected in the filtrate, the bright-green powder, after drying at 212° until it ceased to lose weight, gave on analysis,

Oxide of copper . . .	67.51
Sulphuric acid . . .	16.98
Water (by difference) .	15.51
	<hr/> 100.00

agreeing very closely with the formula $3\text{CuO}, \text{CuO SO}^3, 4\text{HO}$, which involves the following numbers:—

4 Oxide of copper . . .	67.63
1 Sulphuric acid . . .	17.04
4 Water	15.33
	<hr/> 100.00*

The copper was estimated by two methods: (1) by dissolving the salt in dilute nitric acid, and precipitating the oxide by a slight excess of potash; and (2) by heating it to whiteness in a platinum crucible. The residue consisted entirely of oxide of copper, and on examination showed no trace of sulphuric acid.

When a solution of hypochlorite of lime is employed in place of the caustic alkali (the sulphate of copper being in excess), no basic sulphate is formed, the green precipitate consisting entirely of the oxychloride, the formula of which is in most instances $3\text{CuO}, \text{CuCl}, 4\text{HO}$, but upon one or two occasions (dependent apparently upon the length of the time of ebullition) of $3\text{CuO}, \text{CuCl}, 6\text{HO}$. This is somewhat remarkable, inasmuch as the native oxychloride (atacamite), which generally contains 4 atoms, sometimes is found with 5; and Berthier many years ago analysed a specimen from Cobija (Bolivia) containing 6 equivalents. The artificial compound obtained by myself, and the natural one examined by Berthier, agree very closely.

	Native.	Artificial.	Calculated.
3 CuO . . .	50.00	49.85	49.56
1 CuCl . . .	28.25	28.02	28.01
6 HO . . .	21.75	22.13	22.43
	<hr/> 100.00	<hr/> 100.00	<hr/> 100.00

When hypochlorite of soda instead of hypochlorite of lime is employed, the same compound (oxychloride of copper) is preci-

* Cu = 31.7.

pitated; but if, in the preparation of the hypochlorite, the alkali is not thoroughly saturated, a precipitate is formed which, as might be expected, contains both basic salts.

Oxychloride of Copper.—The precipitates produced either by the caustic alkalies or the alkaline hypochlorites (the copper salts being in excess) appear under most circumstances to be identical, viz. $\text{CuCl}, 3\text{CuO}, 4\text{HO}$; but other oxychlorides seem to exist. Gladstone observed, in his experiments upon the hydration of chloride of copper, that, under certain conditions, a precipitate was occasioned which on analysis yielded $2\text{CuO}, \text{CuCl}, \text{HO}$.

Basic Nitrate of Copper ($4\text{CuO}, \text{NO}^5, 3\text{HO}$) or ($\text{CuO}, \text{NO}^5, 3\text{CuO HO}$), and, according to Graham, ($3\text{CuO}, \text{NO}^5, \text{HO}$).—This compound may be obtained either by heating the ordinary nitrate of copper, or by the partial decomposition of its solution by the caustic alkalies. When the hypochlorites are added to nitrate of copper, none of this salt is produced, the precipitate consisting entirely of the oxychloride.

I succeeded some years ago in obtaining delicate crystals of this salt by exposing nitrate of copper to a comparatively gentle heat for many weeks. The resulting mass, on being broken, displayed in the centre a mass of brilliant prisms of a bluish-green colour, perfectly insoluble in water. Analysis yielded 66.48 per cent. oxide of copper, agreeing with the formulæ of Gerhardt and Gladstone.

	Calculated.	Found.
4CuO . . .	66.22	66.48
1NO ⁵ . . .	22.51	
3HO . . .	11.27	
	<hr/> 100.00	

The action of the alkalies and alkaline hypochlorites upon the sulphate, nitrate, and chloride of copper appears to be as follows:—

When potash is added to an excess of sulphate of copper, the compound

$3\text{CuO}, \text{CuO SO}^3, 4\text{HO}$ is produced.

When an alkaline hypochlorite is added,

$3\text{CuO}, \text{CuCl}, 4\text{HO}^*$ is formed.

When potash is added to chloride of copper,

$3\text{CuO}, \text{CuCl}, 4\text{HO}$ is formed.

And the same compound is precipitated by the addition of an alkaline hypochlorite. Potash added to the nitrate of copper

* Under certain circumstances,

$3\text{CuO}, \text{CuCl}, 6\text{HO}$.

precipitates the basic salt,



while the hypochlorites throw down



When potash in the cold is added *in excess* to any of the soluble salts of copper, the pale-blue hydrated oxide is precipitated, which blackens slightly at 120°F. , and becomes quite dehydrated at 212° . It is very singular that when metallic copper is dissolved by means of hydrochloric acid and chlorate of potash in the cold, and potash added to the liquid still holding the oxides of chlorine in solution, the oxide of copper is precipitated as a black powder, even at the ordinary temperature.

XVII. *On Ampèrian Repulsion.*

By MM. J. G. S. VAN BREDA and W. M. LOGEMAN.

To Dr. Tyndall, F.R.S.

SIR,

WE take advantage of the opportunity afforded by the sending of the accompanying letter, to address, through your kindness, to the readers of the *Philosophical Magazine* a few lines in answer to the remarks published by Mr. Croll of Glasgow, in the Number of that Journal for this month, pp. 365 *et seq.*, in reference to our experiments on Ampèrian repulsion.

There must be, either in the description of our experiments, or in the figure which accompanies them, some want of accuracy or of clearness; otherwise we are at a loss to understand how Mr. Croll could say that the currents in the mercury ought to repel the horizontal parts, while they attract the vertical parts of our moveable conductor. This seems to us to contradict all the received ideas of electric currents. We would willingly take trouble to make our description more clear or more accurate, but the remainder of Mr. Croll's article induces us to think that such an attempt would be quite useless.

In fact, Mr. Croll first admits that "our experiments with the reversed conductor, and some others detailed by us, certainly prove that the molecules of a conductor, while the current is passing, *do repel each other*;" and then goes on to discuss the question from a point of view which is new to us, adding that this repulsion of the molecules of a conductor proves nothing as to the repulsion of the contiguous parts of the current itself. Now we will not follow Mr. Croll into the arguments upon which he bases his opinion concerning the mutual action of the parts of

the current ; in the first place, because these arguments appear to us to be beyond the reach of experimental control ; and in the second place, because we think it can be of but slight use to science to speculate on the properties of a current hypothetically detached from the material substratum without which we are unable really to conceive of its existence.

We have the honour to remain, &c.,

Teylerian Laboratory, Haarlem,
May 1862.

J. G. S. VAN BREDa,
W. M. LOGEMAN.

XVIII. *On the Existence of a Symbolic and Biquadratic Equation which is satisfied by the Symbol of Linear or Distributive Operation on a Quaternion.* By Sir WILLIAM ROWAN HAMILTON, LL.D. &c.*

1. **A**S early as the year 1846, I was led to perceive the existence of a certain *symbolic and cubic equation*, of the form

$$0 = m - m'\phi + m''\phi^2 - \phi^3, \quad (1)$$

in which ϕ is used as a symbol of *linear and vector operation* on a *vector*, so that $\phi\rho$ denotes a vector depending on ρ , such that

$$\phi(\rho + \rho') = \phi\rho + \phi\rho', \quad (2)$$

if ρ and ρ' be any two vectors ; while $m, m',$ and m'' are *three scalar constants*, depending on the *particular* form of the linear and vector function $\phi\rho$, or on the (scalar or vector) constants which enter into the composition of that function. And I saw, of course, that the problem of *inversion* of such a *function* was at once given by the formula

$$m\phi^{-1} = m' - m''\phi + \phi^2, \quad (3)$$

—the required assignment of the inverse function, $\phi^{-1}\rho$, being thus reduced to the performance of a limited number of *direct operations*.

2. Quite recently I have discovered that the far more general *linear* (or *distributive*) and *quaternion function of a quaternion* can be *inverted*, by an analogous process, or that there always exists, for any *such* function $f q$, satisfying the condition

$$f(q + q') = f q + f q', \quad (4)$$

where q and q' are any two quaternions, a *symbolic and biquadratic equation*, of the form

$$0 = n - n'f + n''f^2 - n'''f^3 + f^4, \quad (5)$$

in which $n, n', n'',$ and n''' are *four scalar constants*, depending on

* Communicated by the Author.

between the summit of the air-bubble and the surface of the liquid has become very small, these molecular movements are no longer able to take place with a velocity corresponding to that with which the bubble rises. Hence, in order that the air which constitutes the bubble may continue to rise and come above the surface of the liquid, it is plain that the liquid must either part or be raised up. Since the beautiful researches of MM. Donny and Henry, there can, however, no longer be any doubt as to the cohesion of liquids being comparable to that of solids; accordingly the stratum of liquid which separates the summit of the bubble of air from the surface, at the moment at which we take the phenomenon into consideration, offers, notwithstanding its extreme thinness, a resistance much too great to allow it to be broken. Consequently this layer is raised up, or, in other words, a liquid film is formed.

I show that, at the end of the phenomenon, this film necessarily forms part of a sphere, but can in no case become a complete hemisphere, although it approaches thereto the more nearly the larger it is. I verify the last result experimentally: I show that when the air-bubble is very small, when, for example, its diameter does not exceed 1 millimetre, the film which it produces at the surface of the liquid is only a very small part of the sphere to which it belongs; but that in proportion as the volume of air is increased the spherical surface approaches more and more nearly to a hemisphere, so that when the diameter of its base exceeds about 3 centimetres it appears to the eye hemispherical. In what follows, I shall always suppose such films to be so large that they may be regarded as sensibly hemispherical.

When two hemispherical films formed at the surface of a liquid touch each other at the base, it is well known that they penetrate each other more or less, but so that the quantities of air which they respectively enclose remain separated from each other by a liquid film or partition. I show that this partition also forms part of a sphere, generally of a different curvature from that of the first two films. Starting from the principle that the pressure exercised by a film of liquid of spherical curvature on the air enclosed by it varies inversely as the radius of curvature of the film, and denoting the radii of the larger film, of the smaller film, and of the partition respectively by ρ , ρ' , and r , I arrive at the formula

$$r = \frac{\rho\rho'}{\rho - \rho'}$$

which gives the radius of the partition when the radii of the two films are known.

In order to complete the study of such a grouping, we have

only further to find the angles which the two films and the partition form with each other at their junction. On this point I have first to remark, that films of liquid cannot meet each other so as to form angles whose edges are linear: the condition of continuity requires that all along the line of junction of our three films a minute mass should be formed, with surfaces highly concave in the direction perpendicular to this line. With soap-water or with the glycerine solution this small mass is much too slender for the eye to be able to distinguish the transverse curvatures; but they may be seen very well in the systems of films obtained with oil in the midst of an alcoholic liquid, in the manner indicated in my Second Series of researches. This being admitted, it will be seen that this small mass of liquid must have a figure of equilibrium proper to itself: its transverse curvature, however, being enormous as compared with its longitudinal curvature, we may disregard the influence of the latter, and reason as if the small liquid mass were straight; but in this case capillary equilibrium evidently requires that the transverse curvatures of the three small surfaces should be the same, whence it follows that the angles formed by the three films must be equal: these three films accordingly meet each other under angles of 120° .

We may also make use of this equality for determining the radius of the partition in terms of the radii of the other two films, in which case we again arrive at the formula already given.

If we suppose a third hemispherical film to attach itself to the other two, the whole group will necessarily contain three partitions, which, upon the same principle, must meet each other under angles of 120° .

From this common value of the angles formed by the junction of all the films under consideration, I deduce a simple graphic method of constructing the base of a system formed by the union of two or three hemispheres with the intermediate partitions, the radii of the hemispheres after their partial interpenetration being assumed to start with.

I verify this construction by the following experiment:—The base in question being drawn on paper in broad lines, the paper is placed on a table and covered with a thin glass plate, whose upper surface is moistened with the glycerine solution; either two or three bubbles of the same solution are next placed on the glass above the parts of the drawing which represent the respective bases of the united hemispheres; the bubbles then immediately form themselves into a partitioned system, and when, by means of a slight artifice described in the memoir, the diameters of the several hemispheres have been modified so as to bring them into accordance with those of the drawing, the base of the system of films so formed is found to be exactly superposable to

the drawing. The drawing, however, having been made upon the supposition of the equality of the angles formed at the junction of the films, this superposition proves that the supposed equality really exists.

In the systems that we have been examining, one and the same edge does not unite more than three films; and in the system formed by the junction of three hemispheres, four edges come together in one point, namely that which unites the three partitions, and those which unite the three hemispheres, two and two. Now, as I have set forth in my Fifth Series of researches, in the systems of films formed within skeletons of iron wire, we never find more than three films coming together at a single edge, nor more than four edges coming together at the same point: these are, then, two general laws of the combinations of films.

I endeavour to find by experiment upon what these laws can depend, and I arrive at the conclusion that the equilibrium of every system of films in which more than three films come together in one edge, or more than four edges unite at the same point, is unstable. The following are two of my experiments on this subject.

1. If an iron-wire skeleton is constructed by the junction of two squares so arranged that two opposite sides of each square bisect two opposite sides of the other at right angles, and if each square be supposed to be occupied by a plane film, the two films will cut each other, forming a straight liquid edge joining the points of intersection of the solid edges: the system so formed will evidently, in consequence of its perfect symmetry, constitute a system of equilibrium; but the solid edge will be the line of junction of four films. Accordingly, when such a skeleton is taken out of the glycerine solution, it is never found to be occupied by the system of films above described; in that which is actually formed there are two curvilinear edges starting from the two points of intersection of the wires and bordering a plane film, while two curved films starting from the solid wires attach themselves to each of these edges; so that the law of three films to a single edge is satisfied. Now the first system being, as I have remarked, a system of equilibrium, and being more simple than the one which is actually formed, we are forced to conclude, from the fact of its never being produced, that it would be unstable.

2. If we imagine twelve plane triangular films starting respectively from the twelve solid edges of a cubic skeleton, and meeting by their apices at the centre of the cube, we shall again have a system which, by reason of its symmetry, is a system of equilibrium. It is easy to see that in a figure so formed, never more

than three films, forming equal angles with each other, would meet in each liquid edge; but eight liquid edges would come together at the central point. Now a cubic skeleton never gives this figure: a thirteenth film, quadrangular in form, takes the place of the central point, and at each of its four corners two of the oblique edges come together; so that at none of them more than four liquid edges meet each other. Here again, then, as in the previous case, the system first supposed, although a figure of equilibrium, and more simple than the one which is actually formed, would be unstable.

It remained to verify the equality of the angles formed at the junction of three films, or of four edges. I show, in the first place, that these equalities are mutual consequences of each other; so that it is sufficient to prove one of them—the second, for example. For this purpose I investigate the common value of the angles contained by the liquid edges, and find it to be 109° and a fraction; I then select from my systems of films those in which all the films are plane, and consequently all the edges straight lines, namely, those formed by the regular tetrahedron, by the right triangular prism with equilateral bases, and by the regular octahedron. In the first system, which has only four liquid edges going from the summits to the centre and joining the films which start from the solid wires, the equality of the angles in question is self-evident by reason of the perfect symmetry of the entire figure.

Among the liquid edges of the second system, that, namely, formed by the triangular prism, there is one which extends between two liquid points, and whose length can be easily measured with the cathetometer. Now by calculating from the previously measured dimensions of my wire skeleton, and from the theoretical value above given for the angles formed by liquid edges, I had found that the length of the edge in question ought to be 42.44 millims., while measurement with the cathetometer gave 42.37 millims., the difference between which and the calculated length may be disregarded.

The third system, that formed by the regular octahedron, contains six equal rectangles, a diagonal of any of which can be easily measured with the cathetometer. In this case I found for the length of this diagonal, by calculating as before from the known size of the wire skeleton and the theoretical value of the angles formed by the liquid edges, 23.16 millims., while direct measurement gave 23.14 millims., the difference being even smaller than in the last case.

The systems of films formed by other skeletons, that is to say, such as contain certain curved films and consequently curvilinear edges, also afford, though not so distinctly, a verification of the

equality of the angles formed by liquid edges which meet at a point. To take a few examples. First, in the system formed by the cubic skeleton, there is, as I have stated above, a small quadrangular film; but since the angles of this film must, in accordance with what precedes, have the value 109° and a fraction, it is evident that its sides must be slightly convex towards the outside; and this is just what happens in the figure which actually forms. Again, the system of films formed by the hexagonal prismatic skeleton contains at its centre a hexagonal film; but the angles of a regular hexagon with rectilinear sides being considerably greater than 109° , namely 120° , it is plain that the sides of this film must be concave, which is really the case. Lastly, the pentagonal prismatic skeleton yields a system of films having at its centre a pentagonal film; and as the angles of a regular pentagon with straight sides are 108° , or very nearly 109° , the sides of the film in question ought not to show any curvature perceptible by the eye; accordingly they appear to be rectilinear.

The foregoing laws being well established, I apply them to another class of complex systems of films, namely, to the froth which forms on certain liquids, such as champagne, beer, &c. This froth, as every one knows, is made up of a multitude of small films or partitions which confine between them small quantities of gas; accordingly, notwithstanding that it all seems to be governed by chance, it ought to obey the laws in question. Consequently these innumerable partitions must necessarily be everywhere joined three by three and at equal angles, and all the edges must be so distributed that four always meet at the same point and make equal angles with each other. I verify these conclusions experimentally, at least so far as regards the number of films which meet in one edge, or the number of edges which meet at one point, by blowing air under the surface of the glycerine solution, thus producing above the liquid an edifice formed of large chambers separated by partitions, just as children do with soap-water. The structure of such an edifice is evidently similar to that of froth, but the size of the partitions of which it is made up makes it possible for the eye to examine the interior.

I next return to the systems of films formed by iron-wire skeletons. Another law, which I enounced in my Fifth Series of researches, is that in all these systems the mean curvature of each film is equal to nothing. All these films are, in fact, in contact with the free atmosphere on both sides, and therefore evidently cannot exert any pressure upon the air either in one direction or in the other—a condition which, according to what is proved in the Fifth Series, further requires that the mean

curvature at each point of the films in question should be equal to nothing. It would be difficult to verify this law very strictly by observation; but it can be seen at least that, whenever a film exhibits a curvature in one direction, it exhibits an opposite curvature in the direction at right angles.

I have also enounced, finally, two other laws in my Fifth Series, and I examine their causes in the memoir of which this is an abstract; but as they are of secondary importance, I will not speak of them here.

Lastly, I examine theoretically and experimentally the manner in which film-systems are formed on wire skeletons. I will confine myself here to a single example, namely, to the formation of the system of films of the six-sided prism, when a skeleton of that form is removed from the liquid, the axis of the prism being kept vertical. At the moment that the lower base is about to emerge from the liquid, the system consists only of six plane films, which occupy respectively the six lateral faces of the prism; and it is obvious that these films cannot have any tendency to enter towards the centre of the skeleton, because they make with each other the angle of exactly 120° , which two films belonging to the same system always make with each other. When the lower base emerges, it remains at first united to the surface of the liquid by a film, which soon contracts at the bottom, closes up and separates from the liquid, and then, as a plane film, comes to occupy the base in question. But this plane film, making thus right angles with the films which occupy the lateral faces, cannot remain in such a position; accordingly it is seen to ascend between the others, bending them in two by drawing them to itself, and thus diminishing them in size; at the same time six new films are formed, starting from the solid wires and ending in the liquid edges which unite the first films two and two. Equilibrium is at last established, when the ascending film has arrived halfway up the prism.

By this examination of the mode of formation of systems of films, I am led to divide them into three classes—namely, *perfect* systems, *imperfect* systems, and *abortive* systems.

In those of the first class, each solid wire serves as a base, along its whole length, for only one film. I call them perfect, because all the films depend from each other throughout their whole extent, and because such of the liquid edges as have one end upon the skeleton start from its extreme points. With a very few exceptions, such systems are formed in the skeletons of all the polyhedra whose dihedral angles are all of them less than 120° ,—such, for instance, as those of the tetrahedron, octahedron, cube, &c.

In some systems, parts of the solid wires of the skeleton serve

as a base for two portions of film at a time. These are the systems of the second class, and are imperfect, because the two portions of film just mentioned are rendered independent of each other by the wire which separates them. Such, for example, are the systems formed by prisms the number of whose lateral faces exceeds six.

Lastly, with other skeletons we never obtain anything but plane films occupying all the faces of the polyhedron except one. These systems belong to the third class. I call them abortive, because all the films of which they are made up are rendered independent of each other, throughout the whole extent of their perimeters, by the solid wires of the skeleton. I have said that one of the faces remains always unoccupied by a film; this is because an opening is necessarily required to allow free entrance to the air. Such abortive systems are formed in the skeletons of all the polyhedra, all of whose dihedral angles are considerably greater than 120° ; I may give as an example the regular icosahedron.

I conclude the original memoir with the announcement of a further series of researches, in which I shall investigate the systems of films formed by wire skeletons from a different point of view; I shall then show that each of these systems of films takes such a form that the sum of the superficial areas of the films is a minimum.

XX. *The Mathematical Theory of the Vibrations of an Elastic Fluid.* By Professor CHALLIS, M.A., F.R.S., F.R.A.S.*

14. **T**HE principal object of my last communication was to show that in hydrodynamics three fundamental equations are necessary and sufficient, and to indicate the principles on which the third rests, and the process of investigating it. These three equations may be reduced by elimination to a single differential equation in which ψ is the principal variable, and the other variables are x , y , z , and t . The general integral of this equation, if it could be obtained, would not only be applicable to particular cases of disturbance of the fluid, but might also give the means of deducing the laws and circumstances of the motion that are independent of arbitrary conditions. But the equation is too complicated to admit of this course being taken; and happily, so far as regards laws and circumstances of the motion that are not arbitrary, other means are available. Before proceeding to point these out, the following remark, by way of illustration, may be found useful. There is, as is well known,

* Communicated by the Author.

an extensive class of problems which admit of being answered by the formation and integration of ordinary differential equations, while at the same time the answers are not given by the general integrals, but by particular solutions deducible from them. These solutions contain no arbitrary *constants*. When the answers are of this kind, they may also be obtained by direct processes, independently of the general integral. In an analogous manner it may be asserted, with respect to hydrodynamical problems, that those laws and relations of the velocity and condensation which are independent of the arbitrary disturbances, and are indicated by expressions which contain no arbitrary *functions*, may be deduced by processes which are independent of the complete integral of the above-mentioned general differential equation. And, conversely, any results strictly deduced from the differential equations, and admitting of interpretation relative to the motion, but containing no arbitrary functions, must be taken as exponents of laws or circumstances of the motion which are not arbitrary, but depend only on the mutual action of the parts of the fluid and its fundamental properties. These remarks may be exemplified as follows, from what has already been proved in the last communication.

15. It was there shown (art. 5) that in all cases of fluid motion we must have

$$\lambda(d\psi) = udx + vdy + wdz,$$

$$\frac{d\psi}{dt} + \lambda \left(\frac{d\psi^2}{dx^2} + \frac{d\psi^2}{dy^2} + \frac{d\psi^2}{dz^2} \right) = 0.$$

Let us now assume that $udx + vdy + wdz$ is an exact differential, or, what is equivalent, that $\lambda = f(\psi, t)$. If we arbitrarily give a specific form to the function f , the second of the above equations will determine by integration the function that ψ is of x, y, z , and t . Each solution thus obtained corresponds to motion submitted to certain arbitrary conditions; and the above reasoning proves that there may be an unlimited number of instances of such motion satisfying the condition of the integrability of $udx + vdy + wdz$. But in art. 11 of the previous communication, a definite result, viz. that the motion is *rectilinear*, is obtained on the supposition of the integrability of that quantity, without assigning any form to the function f , that is, without imposing on the fluid any arbitrary conditions. That result must, therefore, be interpreted with reference to the mutual action of the parts of the fluid; and the fact of its having been shown to be a consequence of the integrability of $udx + vdy + wdz$ is in conformity with the principle enunciated in art. 9, that every analytical circumstance of a general character corresponds to a general characteristic of the motion, and the converse.

Since, however, the result was not deduced from the combination of all the three general equations, we are not entitled to conclude that all motion which depends only on the mutual action of the parts of the fluid is rectilinear, but that rectilinear motion depending on such action is a law of the motion of an elastic fluid.

16. As this law of rectilinear motion is of prime importance, it will be worth while to endeavour to place in as clear a light as possible the reasoning by which it was deduced. In art. 11 the line s is defined to be an orthogonal trajectory between a given surface of displacement from which the trajectories originate, and any other surface of displacement; and it was shown that $(ds)=0$, if the differential be taken from point to point of the latter surface. Now possibly it may be thought that $ds=0$, at a given time, merely because the lines s are by hypothesis perpendicular to that surface, and that the parallelism of the surfaces of displacement does not flow from this result. The following simple instance will serve to clear up this point. Let the surfaces of displacement be planes passing through a fixed axis. Then the orthogonal trajectories will be circular arcs; and if θ be any finite angle made by two of the surfaces, we shall have, at the distance r from the axis, $s=r\theta$, and, at the same time, at the distance $r+dr$, $s+ds=(r+dr)\theta$. Hence $ds=\theta dr$; and s cannot be constant in passing from one trajectory to another more distant from the axis, so long as the surfaces are inclined to each other.

17. These preliminaries being settled, we have next to inquire what particular rectilinear motion satisfying the condition of the integrability of $udx+vdy+wyz$ the fluid admits of, independently of the character of the arbitrary disturbances. For instance, that function would be integrable *per se* if the motion were in lines perpendicular to a plane. But when we have obtained, by strict analytical reasoning, equations derived from the hypothesis of such rectilinear motion, we find that they give contradictory results, and admit of no interpretation relative to the motion of an elastic fluid. I allude to the known equations

$$w=a \text{ Nap. log } \rho=f(z-(a+w)t),$$

from which, as I have shown in vol. xxxii. S.3. of the Philosophical Magazine (p. 496), the inference may be drawn, that points of no velocity and condensation may at the same instant be points of maximum velocity and condensation. This contradiction merely shows that the rectilinear motion which is the subject of the present inquiry (that, namely, which is due to the mutual action of the parts of the fluid) is not of the supposed kind. If no contradiction had been met with, we must have concluded

that the *free* motion of an elastic fluid was parallel rectilinear motion, because from the nature of the case that kind of motion must be unique and definite in its character.

18. It may, however, be urged *per contra*, that parallel rectilinear motion may certainly be impressed on the fluid under arbitrary circumstances, and that the investigation of the relation of the velocity to the density would, in a particular case of disturbance, lead to the very equations adduced above, which therefore must admit of interpretation relative to fluid motion. To this argument I reply that the investigation of the laws of the mutual action of the parts of the fluid must *precede* the consideration of particular cases of motion, and that, by taking this course, it will be found that the above equations are not applicable to any instance of parallel rectilinear motion arbitrarily impressed on the fluid. To this point I shall recur in a subsequent stage of the reasoning.

19. Again, $udx + vdy + wdz$ would be integrable *per se* if the motion were in straight lines drawn from a centre. It will spare the necessity of any lengthened consideration of this case, to say that if the equations derived from the hypothesis of this kind of rectilinear motion involved no contradiction, neither would there be contradiction in the equations just considered, because parallel rectilinear motion may be regarded as central rectilinear motion at an infinite distance from the centre. We must conclude that as the one hypothesis led to contradictions, so would the other, and consequently that the kind of rectilinear motion resulting from the mutual action of the parts of the fluid is not central rectilinear motion.

20. There remains to be made the hypothesis of an *axis* of rectilinear motion; that is, of motion in a straight line coexisting with curvilinear motion. That this is a legitimate hypothesis will appear from the consideration that, as the law of rectilinearity was not an inference from a combination of all the general equations, we are not under the necessity of supposing the motion to be wholly rectilinear. Now, since there are no arbitrary conditions to take into account, we are at liberty to suppose the axis of rectilinear motion to be one of the axes of coordinates, for instance the axis of z ; and we have then to express analytically that the motion parallel to the plane xy vanishes for points on this axis, and that, for points immediately contiguous to it, $udx + vdy + wdz$ is an exact differential. These conditions may be satisfied as follows. Let f be a function of x and y , and ϕ a function of z and t , and, since the above expression is to be an exact differential, assume that

$$(d \cdot f\phi) = udx + vdy + wdz.$$

Then

$$u = \phi \frac{df}{dx}, \quad v = \phi \frac{df}{dy}, \quad w = f \frac{d\phi}{dz};$$

and the required conditions are satisfied if, where $x=0$ and $y=0$, we have also $\frac{df}{dx}=0$ and $\frac{df}{dy}=0$, and consequently the value of f a maximum or minimum. If f be a maximum, the velocity estimated parallel to the axis of z is also a maximum along that axis; and since this, as we shall afterwards see, must be the case, the minimum value is excluded.

21. The next step in the investigation of the character of the free motion of the fluid is to substitute the above values of u , v , w in the two general equations (1) and (2) in art. 3, and to eliminate ρ between them. The equation resulting from this elimination is given under Prop. X., in a communication "On the Principles of Hydrodynamics" in the Philosophical Magazine for December 1852. As, however, the condition of the integrability of $udx + vdy + wdz$ applies only to points immediately contiguous to the axis of z , we must omit in that equation terms involving $\frac{df}{dx}$ and $\frac{df}{dy}$; and then, supposing there are no extraneous forces, it reduces itself to the following:

$$F'(t) = a^2 \phi \left(\frac{d^2 f}{dx^2} + \frac{d^2 f}{dy^2} \right) + a^2 f \frac{d^2 \phi}{dz^2} - f \frac{d^2 \phi}{dt^2} - 2f^2 \frac{d\phi}{dz} \cdot \frac{d^2 \phi}{dz dt} - f^3 \frac{d\phi^2}{dz^2} \cdot \frac{d^2 \phi}{dz^2}.$$

If we suppose f to have a minimum value where $x=0$ and $y=0$, the multiplier of ϕ in the above equation will be a positive quantity, and the integration of the equation will lead to logarithmic functions admitting of no interpretation relative to the motion. We must, therefore, suppose f to have a maximum value, which, if we please, may be unity. Then we shall also have by the conditions of a maximum,

$$\frac{d^2 f}{dx^2} + \frac{d^2 f}{dy^2} = -\frac{b^2}{a^2},$$

b^2 being some constant as yet unknown. Again, as the present investigation is independent of any arbitrary condition, the arbitrary function $F'(t)$ has no signification, and we may therefore suppose it to be zero, or $F(t)$ to be a constant. The equation then becomes

$$0 = b^2 \phi - a^2 \frac{d^2 \phi}{dz^2} + \frac{d^2 \phi}{dt^2} + 2 \frac{d\phi}{dz} \cdot \frac{d^2 \phi}{dz dt} + \frac{d\phi^2}{dz^2} \cdot \frac{d^2 \phi}{dz^2}. \quad (5)$$

22. The integration of this equation may be expected to give

the laws of the free motion which takes place along the axis of rectilinear motion; and as no case of arbitrary disturbance is under consideration, the steps of the analytical reasoning should conduct to a function of explicit form indicative of a special kind of motion. We have next to inquire whether such a result is deducible, and by what process. First, it may be remarked that, as the equation seems not to admit of being integrated when all the terms are retained, we must proceed by successive approximations, commencing with the terms of the first order with respect to ϕ . Thus we shall have to integrate

$$\frac{d^2\phi}{dt^2} - a^2 \frac{d^2\phi}{dz^2} + b^2\phi = 0. \quad . \quad . \quad . \quad . \quad (6)$$

It does not appear that the general integral of this equation can be expressed in a finite number of terms; the only form it is known to admit of is the following, in which, for the sake of brevity, e is put for $\frac{b^2}{4a^2}$, u for $z - at$, and v for $z + at$:

$$\begin{aligned} \phi = & F(\mu) + evF_1(\mu) + \frac{e^2v^2}{1 \cdot 2} \cdot F_2(\mu) + \frac{e^3v^3}{1 \cdot 2 \cdot 3} \cdot F_3(u) + \&c. \\ & + G(v) + e\mu G_1(v) + \frac{e^2\mu^2}{1 \cdot 2} \cdot G_2(v) + \frac{e^3\mu^3}{1 \cdot 2 \cdot 3} \cdot G_3(v) + \&c., \end{aligned}$$

where

$$F_1(\mu) = \int F(\mu) d\mu, \quad F_2(\mu) = \int F_1(\mu) d\mu, \quad \&c.,$$

and

$$G_1(v) = \int G(v) dv, \quad G_2(v) = \int G_1(v) dv, \quad \&c.$$

As the arbitrary functions F and G satisfy the equation independently, we are at liberty to consider them separately. Suppose G to disappear. Then the value of ϕ has this peculiarity, that it is an expansion by Taylor's theorem of a finite expression, if forms of F can be found which satisfy the equality

$$\frac{d \cdot F_n(u)}{d\mu} = \pm k^2 F_{n+1}(u)$$

for every integral value of n . Now since $F_{n+1}(u) = \int F_n(u) d\mu$, it follows from the above equality that

$$\frac{d^2 \cdot F_n(u)}{d\mu^2} = \pm k^2 \cdot F_n(u).$$

As the upper sign would lead to a logarithmic form of F , which is incompatible with any general law of fluid motion, we must take the lower, and shall then have by integration,

$$F_n(u) = A \cos(k\mu + B).$$

Regard being had to the meaning of the suffix n , this result shows that we may suppose that $F(\mu) = m \cos(k\mu + c)$: whence it will be found that

$$\phi = m \cos \left\{ k \left(\mu - \frac{ev}{k^2} \right) + c \right\};$$

or putting q for $k - \frac{e}{k}$, and substituting the values of μ and ν ,

$$\phi = m \cos q \left(z - at \sqrt{1 + \frac{4e}{q^2}} + c' \right).$$

The consideration of the function G by itself would similarly conduct to the same form of ϕ , with the difference only of a positive sign before at , indicative of propagation in the opposite direction. We may hence infer the possibility of the coexistence of two series of waves propagated in opposite directions.

Thus we have been led, step after step, by the indications of the analysis, to a special and definite expression for the function ϕ ; and as this result is antecedent to any supposed disturbance of the fluid, it must be taken to be expressive of a law of the mutual action of the parts of the fluid. Further, it may be remarked that every finite expression involving trigonometrical functions is to be regarded as an exact algebraic expression, because, as is known, these functions all admit of being expressed in terms of exponential quantities. Thus the particular solution of (6) which we have obtained is finite and exact, and in this respect stands out from all particular integrals of the same equation, which, as the foregoing reasoning shows, can only be expressed in infinite series. Upon the general principle previously enunciated, this analytical circumstance is significant, and may be interpreted as meaning that the fluid is susceptible, by its own action, of a special kind of motion of which this unique result is the exponent.

23. Another inference of a remarkable character may be drawn from the above particular solution, viz. that *there is no other solution of equation (5) which gives a constant velocity of propagation.* This theorem admits also of direct proof as follows. Let us suppose for the moment that $\phi = F(z - a_1 t)$, a_1 being some constant. Then we have

$$\frac{d\phi}{dz} = F'(z - a_1 t), \quad \frac{d\phi}{dt} = -a_1 F'(z - a_1 t).$$

But for the motion under consideration along the axis of z , the general equation (1) becomes

$$a^2 \text{Nap. log } \rho + \frac{d\phi}{dt} + \frac{d\phi^2}{2dz^2} = 0;$$

whence it follows that ρ is also a function of $z - a_1 t$. Thus the above supposition is equivalent to assuming that a given state of

velocity and density at any point of the axis of rectilinear motion is propagated with the uniform velocity a_1 . Let us introduce this condition into the equation (5) by substituting $F(z-a_1t)$ for ϕ . Putting for brevity μ for $z-a_1t$ and F for $F(\mu)$, the result is

$$\frac{d^2F}{d\mu^2} \left(a_1^2 - a^2 - 2a_1 \frac{dF}{d\mu} + \frac{dF^2}{d\mu^2} \right) + b^2F = 0. \quad (7)$$

Integrating now to the first approximation, the value obtained for ϕ is precisely that given by the very different process in art. 22. This proves that no other form of solution is compatible with uniform propagation. It is observable that if the equation (5) had not contained the term $b^2\phi$, the integration to the first approximation would have given a uniform velocity of propagation equal to a , independently of the form of the function ϕ .

24. In the communication contained in the Philosophical Magazine for December 1852, I have gone through a somewhat intricate process of analytical reasoning to prove that, on the hypothesis of uniform propagation,

$$\frac{d\phi}{dt} + a_1 \frac{d\phi}{dz} = 0,$$

and, by consequence, that the *exact* value of ϕ is a function of $z-a_1t$. The same result is more simply deduced from the equation (7), which shows that, however far the approximation be carried, F or ϕ is a function of $z-a_1t$ of specific form. In the communication just referred to, I have integrated that equation to terms inclusive of the third power of m , and obtained the value of a_1 to terms inclusive of the square of m . Putting for shortness μ' for $z-a_1t+c$, the following are the results:—

$$\phi = m \cos q\mu' - \frac{m^2 q^3 a_1}{3b^2} \sin q\mu' - \frac{m^3 q^4}{4b^2} \left(\frac{q^2 a_1^2}{b^2} - \frac{1}{8} \right) \cos 3q\mu', \quad (8)$$

$$a_1^2 = a^2 + \frac{b^2}{q^2} + m^2 q^2 \left(\frac{2q^2 a^2}{3b^2} + \frac{5}{12} \right). \quad (9)$$

Hence it appears, as was remarked at the end of my communication to the Philosophical Magazine for last April, the rate of propagation is not altogether independent of the maximum velocity of vibration.

25. The next proposition, the proof of which is required by the course of the present investigation, may be thus enunciated:—Terms of the first order being alone taken into account, the function $u dx + v dy + w dz$ is an exact differential for points at any distance from the axis of rectilinear motion. This proposition may be proved as follows. Let the pressure at the point xyz at

the time t be $a^2(1 + \sigma)$, σ being a small quantity the powers of which above the first are neglected. Then, by known equations,

$$\frac{a^2 d\sigma}{dx} + \frac{du}{dt} = 0, \quad \frac{a^2 d\sigma}{dy} + \frac{dv}{dt} = 0, \quad \frac{a^2 d\sigma}{dz} + \frac{dw}{dt} = 0.$$

From these equations it is easily seen that

$$\frac{d^2 u}{dt dy} - \frac{d^2 v}{dt dx} = 0, \quad \frac{d^2 u}{dt dz} - \frac{d^2 w}{dt dx} = 0, \quad \frac{d^2 v}{dt dz} - \frac{d^2 w}{dt dy} = 0.$$

Hence by integration;

$$\frac{du}{dy} - \frac{dv}{dx} = C, \quad \frac{du}{dz} - \frac{dw}{dx} = C', \quad \frac{dv}{dz} - \frac{dw}{dy} = C'',$$

the functions C, C', C'' being independent of the time. But from what has already been ascertained respecting the motion which is the subject of this investigation, we may infer that it is *vibratory*, so that neither u, v, w nor their partial differential coefficients with respect to coordinates can be independent of the time. Hence $C=0, C'=0, C''=0$, and consequently $udx + vdy + wdz$ is an exact differential. The same proposition is proved in a somewhat different manner in the communication of December 1852.

26. This theorem being established, it will not be necessary, after assuming $udx + vdy + wdz$ to be an exact differential, to restrict the reasoning to positions immediately contiguous to the axis of rectilinear motion. Having regard, however, to the antecedent general demonstration of the existence of such an axis, we shall still assume that

$$(d \cdot f\phi) = udx + vdy + wdz,$$

f being a function of x and y , and ϕ a function of z and t . The legitimacy of the assumption will appear in the sequel of the reasoning. Thus we have, as before,

$$u = \phi \frac{df}{dx}, \quad v = \phi \frac{df}{dy}, \quad w = f \frac{d\phi}{dz}.$$

Also the equation which gives the condensation σ to the first order of approximation is

$$a^2 \sigma + f \frac{d\phi}{dt} = 0,$$

the arbitrary function of the time being caused to vanish on the principle, already adopted, of conducting the reasoning independently of any arbitrary conditions. By eliminating σ between this equation and the second general equation taken to the first order of approximation, viz.

$$\frac{d\sigma}{dt} + \frac{du}{dx} + \frac{dv}{dy} + \frac{dw}{dz} = 0,$$

the result is

$$a^2\phi\left(\frac{d^2f}{dx^2} + \frac{d^2f}{dy^2}\right) + a^2f\frac{d^2\phi}{dz^2} - f\frac{d^2\phi}{dt^2} = 0.$$

But we have already found for points on the axis that

$$b^2\phi - a^2\frac{d^2\phi}{dz^2} + \frac{d^2\phi}{dt^2} = 0.$$

And as ϕ is by hypothesis independent of x and y , its value is the same in this as in the preceding equation. Hence by substituting in the latter from the former, and striking out the common factor ϕ , we obtain

$$\frac{d^2f}{dx^2} + \frac{d^2f}{dy^2} + \frac{b^2f}{a^2} = 0. \quad . \quad . \quad . \quad . \quad . \quad (10)$$

Since, according to this equation, f may be a function of x and y only, the original assumption respecting this function and ϕ is proved to be legitimate.

27. Because $\sigma = -\frac{f}{a^2} \cdot \frac{d\phi}{dt}$, it appears that for given values of z and t , the condensation is proportional to f . By substitution in the equation (8) and striking out common factors, there results the equation

$$\frac{d^2\sigma}{dx^2} + \frac{d^2\sigma}{dy^2} + \frac{b^2\sigma}{a^2} = 0. \quad . \quad . \quad . \quad . \quad . \quad (11)$$

28. Since the equation (10) is of the same form as (6), the process applied to the latter equation will also give a particular solution of the former. Substituting $4e$ for $\frac{b^2}{a^2}$, the solution thus obtained is

$$f = \alpha \cos 2\sqrt{e}(x \cos \theta + y \sin \theta),$$

which evidently satisfies (10), α and θ being arbitrary constants. The meaning of this result is, that the motion is wholly parallel to a plane making an angle θ with the axis of x . It must not, however, be concluded that this is a general law to which motion depending on the mutual action of the parts of the fluid is subject, simply for the reason that θ is an arbitrary quantity. According to the principle which has all along governed the investigation, we must, as far as is possible, get rid of any arbitrariness in the integral. This may readily be done by observing that since the equation (10) is linear with constant coefficients, it is satisfied by supposing that

$$f = \Sigma . \alpha \delta \theta \cos 2\sqrt{e}(x \cos \theta + y \sin \theta),$$

$\delta \theta$ being an indefinitely small constant angle, and the summation being taken from $\theta = 0$ to $\theta = 2\pi$, in order to embrace every

possible position of the above-mentioned plane. By performing the summation, substituting r^2 for $x^2 + y^2$, and determining α so as to satisfy the condition that $f=1$ where $r=0$, the result is

$$f=1 - er^2 + \frac{e^2 r^4}{1^2 \cdot 2^2} - \frac{e^3 r^6}{1^2 \cdot 2^2 \cdot 3^2} + \&c. \quad . \quad . \quad (12)$$

This value of f , containing no arbitrary quantity whatever, expresses the general law of the arrangement of the condensation, in the case of free motion, about the axis of rectilinear motion. It is evident that the same result might be obtained from (10) by supposing, as might be presumed to be the case in free and undisturbed motion, that the condensation σ , and therefore f , is a function of r . I have pointed out other methods of obtaining the series (12) in the communication of December 1852 before referred to.

29. To ascertain the precise character of the free vibrations of an elastic fluid, it will now be necessary to discuss the properties of the function f . First, it is to be observed that, since the condensation in any plane transverse to the axis of z is proportional to f , those values of r which make f vanish are radii of cylindrical surfaces in which the condensation is zero throughout the motion. Similarly, those values of r which make $\frac{df}{dr}$ vanish are radii of cylindrical surfaces in which the transverse velocity is always zero. In order to calculate the magnitudes of these different radii, it is necessary first to determine the value of the unknown constant e . I have given a process for effecting this determination in a communication to the Philosophical Magazine for February 1853, under Prop. XI.; and no other method of obtaining the same result has since occurred to me, although I can scarcely doubt that the problem admits of being solved in a different manner. At the same time I have seen no reason to call in question the exactness of the solution there given, which, as it is important, and admits of being exhibited in a somewhat simpler form, I shall now repeat.

30. Assuming f to be a function of r , the equation (10) becomes

$$\frac{d^2 f}{dr^2} + \frac{df}{r dr} + 4ef = 0, \quad . \quad . \quad . \quad . \quad (13)$$

and the equation (12) is the integral of this equation in a series. The above-mentioned process for finding the value of e requires the determination of values of r which make f vanish at very great distances from the axis; and at first sight it might be supposed that for very large values of r the middle term of the above equation might be omitted, and the function that f is of r be obtained with sufficient approximation by integrating the equation

deprived of this term. But to this proceeding there lies the objection, that where the middle term is a maximum, $\frac{d^2f}{dr^2}$ is equal to $\frac{df}{rdr}$, however large r may be. Thus, since at very remote distances from the axis there may be values of the first and second terms comparable with each other, the latter term cannot be legitimately omitted for large values of r .

Substituting now for convenience x^2 for er^2 , the general or n th term of the right-hand side of the equation (12) is, irrespective of the sign,

$$\frac{x^{2n-2}}{1^2 \cdot 2^2 \cdot 3^2 \dots (n-1)^2}.$$

If this be greater than the $n+1$ th term, x^2 is less than n^2 ; and if it be greater than the $n-1$ th term, x^2 is greater than $(n-1)^2$. If $x^2 = n^2$, the n th and $n+1$ th terms are equal to each other, and greater than any preceding or following term. This being premised, let us next give to the equation (12) the form

$$f = \Sigma \cdot \frac{(-1)^{n-p+1}}{1^2 \cdot 2^2 \cdot 3^2 \dots n^2} \times \left\{ \begin{array}{l} n^{2p+2} \cdot \left(1 - \frac{1}{n}\right)^2 \left(1 - \frac{2}{n}\right)^2 \dots \left(1 - \frac{p}{n}\right)^2 x^{2n-2p-2} \\ - n^{-2p} \cdot \left(1 + \frac{1}{n}\right)^{-2} \left(1 + \frac{2}{n}\right)^{-2} \dots \left(1 + \frac{p}{n}\right)^{-2} x^{2n+2p} \end{array} \right\},$$

in which n may be supposed to be any integer. By putting for p in the first term within the brackets the values $n-1, n-2, \dots, 2, 1, 0$, all the terms to the n th inclusive are obtained; and by putting for p in the other term the values $0, 1, 2, \dots$ *ad infinitum*, all the remaining terms are obtained. Let us now suppose that x is very large, and that it has the particular value n_1 which makes the two largest terms cancel each other. Then the largest terms will be very distant from the first term. The expression for f above shows that the same value n_1 of x which makes the two largest terms disappear, cancels also all terms of the next inferior order with respect to n_1 , so that for this value the equation becomes, as far as regards the terms which can be paired together, of the form

$$f = An_1^{2n_1-2} + Bn_1^{2n_1-3} + \&c.,$$

the first term of which is removed by two degrees from the greatest terms. With respect to the terms which cannot be paired, it is evident that they will be comparatively very small on account of their distance from the largest terms, and the small

magnitude of the product

$$\left(1 + \frac{1}{n_1}\right)^{-2} \left(1 + \frac{2}{n_1}\right)^{-2} \dots \left(1 + \frac{p}{n_1}\right)^{-2},$$

when p exceeds $n_1 - 1$. It has thus been shown that the value $x = n_1$ satisfies the equation $f = 0$ approximately, and that the true root differs so much the less from this as x is greater. Ultimately, therefore, $r_1 \sqrt{e} = n_1$, and the condensation is zero in a cylindrical surface of which the radius is r_1 . As n_1 is any very large integer, we shall equally have $r_2 \sqrt{e} = n_1 + 1$, r_2 being the radius of the next greater cylindrical surface of no condensation. Hence if D = the common interval between the consecutive surfaces, $D \sqrt{e} = 1$.

Again, the transverse vibrations at great distances from the axis are performed in the same manner, and in the same time, as vibrations along the axis which would result from two equal series of waves propagated simultaneously in opposite directions. In both cases there will be *nodes* and *loops* separated by constant intervals. Hence the intervals between consecutive points of no condensation must be the same in both, the times of vibration being the same. As we found that $\phi = m \cos q(z - a_1 t + c)$, the interval for the longitudinal vibrations will be, as is known, $\frac{\pi}{q}$; and the interval for the transverse vibrations being $\frac{1}{\sqrt{e}}$, it fol-

lows that $\sqrt{e} = \frac{q}{\pi}$. Or, since $\sqrt{e} = \frac{b}{2a}$, we have $\frac{b}{2a} = \frac{q}{\pi}$, and consequently $\frac{b^2}{q^2} = \frac{4a^2}{\pi^2}$. The first approximation to the velocity of propagation obtained in art. 24 is thus shown to be $a \sqrt{1 + \frac{4}{\pi^2}}$.

31. By substituting for b^2 and a_1 from the above results in the equations (8) and (9), and putting $\frac{2\pi}{\lambda}$ for q , and m' for $\frac{2\pi m}{\lambda}$, it will be found that

$$\frac{d\phi}{dz} = m' \sin \frac{2\pi\mu'}{\lambda} - \frac{\pi(\pi^2 + 4)^{\frac{1}{2}} m'^2}{6a} \cos \frac{4\pi\mu'}{\lambda} - \frac{3\pi^2(2\pi^2 + 7)m'^3}{128a^2} \sin \frac{6\pi\mu'}{\lambda}$$

$$a_1^2 = a^2 \left(1 + \frac{4}{\pi^2}\right) + \frac{m'^2}{12} (2\pi^2 + 5).$$

Having thus found the forms of the functions ϕ and f , and the value of the assumed constant b^2 , the free vibratory motion of an elastic fluid is completely determined, so far as regards vibrations of small magnitude. In another communication I

propose to advert to some remaining points of this mathematical theory which may require elucidation, and to draw some inferences from the foregoing results, particularly with reference to the undulatory theory of light.

Before concluding this communication, I beg to advert to the reference made by Prof. Tyndall, in the *Philosophical Magazine* for last April, to the discussion which, as he says (§ 2), was "carried on in 1851 between Professors Challis and Stokes regarding Laplace's correction for the theoretic velocity of sound." From his expressing a hope that I shall deem conclusive the results of the experiments on radiant heat to which he refers, I hardly think he can be fully aware of the points involved in that discussion. The results of experiments made by so able a physicist I accept as valuable and trustworthy; but I fail to see how those to which he directs my attention settle the question about the velocity of sound, the precise nature of which will perhaps be understood by the following statement. Laplace, who, it is well known, did not give much attention to the physical applications of partial differential equations, was content to deduce from hydrodynamics the value of the velocity of sound first obtained by Newton. In this he has been followed by later mathematicians, as Poisson and others, who, in deducing the rate of propagation, have adopted a process first suggested by Lagrange. After much consideration given for a long time to the mathematical theory of hydrodynamics, I discovered that that process is objectionable in principle, and that by the application of correct mathematical principles (which are fully explained in this communication) a value of the velocity is obtained so accordant with that given by observation as to leave scarcely any residual quantity to account for. This is the real question at issue between me and Prof. Stokes, who contends for the Newtonian value as ordinarily deduced; and it will thus be seen that, antecedently to any physical considerations, there is a question to be settled which is exclusively a mathematical one.

With respect to the attempt made by Laplace to account for the difference between the Newtonian and the observed velocities of sound, I fully believe that such a theory as that which he proposed would never have been thought of if the principles of the application of partial differential equations to the determination of the motion of fluids had been better understood. Laplace's physical theory has not been admitted in all particulars; but the general conception that the heat developed by sudden condensation, or absorbed by sudden rarefaction, produces the excess of velocity, is accepted. To make, however, this general view bear upon the fact, three distinct hypotheses have been made:—(1) that a certain amount of increment of temperature always

accompanies a certain amount of condensation, whatever be the neighbouring condensations, and whether the waves be long or short, and the excursions of the particles be swift or slow, and that it is exactly proportional to the condensation; (2) that this increment of temperature is acquired simultaneously with the condensation; (3) that the increment of temperature is *quam proxime* the same in unconfined air as when the air suffers the same amount of condensation in a confined space. Since these laws are not deduced from acknowledged principles, they can have no other basis than experiment to rest upon. I am not, however, aware of any experiments which lend them support, unless the new experiments of Prof. Tyndall justify the third hypothesis. But I confess I do not yet see in what way they do this, or, if they give countenance to this hypothesis, that the theory is made good, so long as the other two hypotheses remain unsupported. I cannot but think that the most probable account of the matter is, that when the same portion of air is continually undergoing condensations and rarefactions in rapid succession, the respective caloric effects are continually neutralizing each other, and thus the rate of the velocity of propagation remains unaltered.

Cambridge, July 9, 1862.

XXI. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 73.]

June 20, 1861.—Major-General Sabine, R.A., Treasurer and Vice-President, in the Chair.

THE following communication was read:—

“On the Measurement of Electric Resistance.” By Professor W. Thomson, F.R.S.

Part I. *New Electrodynamic Balance for resistances of short bars or wires.*

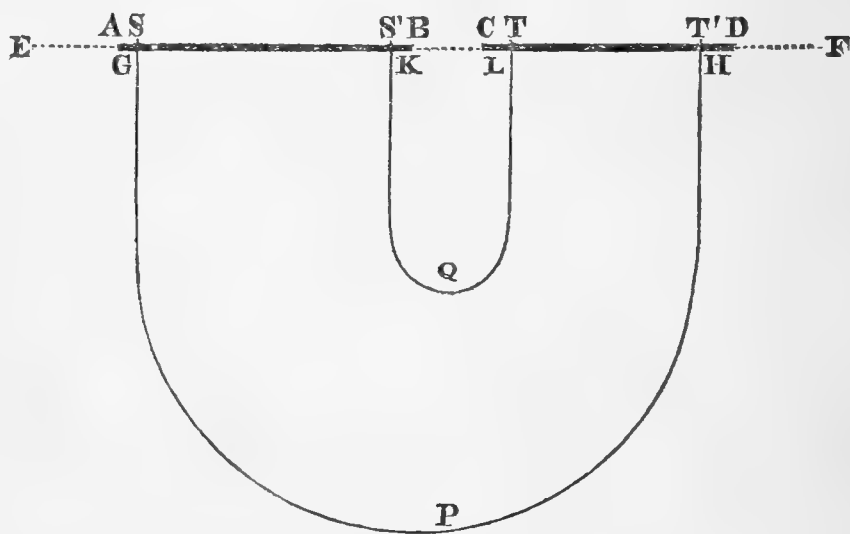
In measuring the resistances of short lengths of wire by Wheatstone's Balance *, I have often experienced considerable difficulty in consequence of the resistances presented by the contacts between the ends of the several connected branches or arcs. This difficulty may generally be overcome by soldering or amalgamating the contacts, when allowable; but even with soldered connexions there is some uncertainty relating to the dimensions of the solder itself, when the wires tested are very short. When soldering was not admissible, I

* I have given this name to the beautiful arrangement first invented by Professor Wheatstone, and called by himself a “differential resistance measurer.” It is frequently called “Wheatstone's Bridge,” especially by German writers. It is sometimes also, but most falsely, called “Wheatstone's Parallelogram.”

have avoided being led into error, by repeating the experiment several times with slightly varied connexions; but I have in consequence sometimes altogether failed to obtain results by either Wheatstone's or any other method hitherto practised, as for instance in attempting to measure the electric resistances of a number of metallic bars each 6 millimetres long and 1 millim. square section, which were put into my hands by Mr. Calvert of Manchester, being those of which he and Mr. Johnstone determined the relative thermal conductivities in their investigation published in the Transactions of the Royal Society for March 1858. I have thus been compelled to plan a new method for measuring electric resistances in which no sensible error can be produced by uncertainty of the connexions, even though made with no extraordinary care.

Let AB and CD be the standard and the tested conductors respectively. Let the actual standard of resistance be the resistance of the portion of AB between marks * S, S' on it, and let it be required to find a portion TT' of CD which has a resistance either equal, or bearing a stated ratio, to that standard.

Join BC either by direct metallic contact between them, or by any ordinarily good metallic connexion with binding screws or otherwise; and join the two electrodes of a galvanic element to their other ends, A, D. Let GPH and KQL be two auxiliary conductors, which, to avoid circumlocutions, I shall call the primary and the



secondary testing-conductors respectively, with their ends applied to the marked points S, T', S', T. Let P and Q be points in these conductors to which the electrodes of the galvanometer are to be applied.

It is easily seen, and will be demonstrated below, that if the resistances of the testing-conductors be similarly divided in Q and P, and if their ends be in perfect conducting communication with the marked points of the main line to which they are applied, the condition that the galvanometer indication may be zero is that the ratio of the resistances of the standard and tested conductors must be the same as that in which the auxiliary conductors are each divided.

* On the same principle as the "mètre à traits" instead of the "mètre à bouts" for a standard of length.

Further, it is clear that by making the testing-conductors of incomparably greater resistances than any that can exist in the connexions at S, S', T, T', which can easily be done if these connexions are moderately good, the error arising from such imperfections as they must present may be made as small as is required*. To demonstrate the above and to form an accurate idea of the operation of this method, it is necessary to investigate the difference of potentials (electromotive force) produced between Q and P, when a stated difference of potentials, E, is maintained between S and T'.

Let SS', TT' denote the resistances between the marks, on the standard and tested conductors respectively. Let GPH, GP, PH, KQL, KQ, QL denote the resistances of the testing-conductors and their parts according to the diagram, implying that

$$GPH = GP + PH, \text{ and } KQL = KQ + QL.$$

Let SG, HT', S'K, LT be the resistances in the connexions at the marks; let

$$SG + GPH + HT' \text{ be denoted by } SPT',$$

and

$$S'K + KQL + LT \quad ,, \quad ,, \quad S'QT;$$

and let S'BCT denote the resistance between S' and T composed of

* This method may be readily applied to Siemens's mercury standards (see Phil. Mag. Jan. 1861, or Poggendorff's 'Annalen,' 1860, No. 5), by introducing platinum wires through holes in the glass tube near its ends, as electrodes for the testing-conductors, and wires or plates of platinum at the ends, as electrodes for one pole of the battery and for connexion with the conductor to be compared with it, respectively. It will then not be the whole line of mercury from end to end, but the portion of it between the two platinum wires first mentioned, that will be the actual standard. The objection against the use of mercury as a standard of resistance, urged by Matthiessen, that the amalgamated copper electrodes which Siemens found necessary to give very perfect *end* connexions must render the mercury impure and increase its resistance sensibly after a time, is thus completely removed. It must be shown, however, that different specimens of commercial mercury, dealt with in the manner prescribed by Siemens, to remove impurities, shall always be found to have equal specific resistances, before his proposal to produce independent standards by filling gauged tubes with mercury can be admitted as valid. But the *transportation and comparison of actual standards* between different experimenters in different places is, and probably must always be, the only way to obtain *the most accurate possible common system of measurement*: and when a proper mutual understanding between electricians and national scientific academies, in all parts of the world, has been arrived at, as it is to be hoped it may be soon, through the assistance of the British Association and Royal Society if necessary, the use of definite metallic standards, whether the liquid mercury as proposed by Siemens, on the one hand, or the solid wire, alloy of gold and silver, on the other hand, proposed by Matthiessen (Phil. Mag. Feb. 1861), would be essential only in the event of all existing standards being destroyed.

Weber's absolute system is often referred to as if its object were merely to fix standards of resistance, and the difficulty and expense of applying it independently have been objected to as fatal to its general adoption. In reality its great value consists in the dynamic conditions which it fulfils, with relation to electro-magnetic induction, and to the mechanical theories of heat and of electro-chemical action. But it most probably will also be much more accurate than any definite metallic convention, for the re-establishment of a common metrical system, in case of the destruction of all existing standards.

the resistance in the connexion and the resistances in the portions of the main conductors from the marks S' and T to their ends. Lastly, let R denote the resistance in the double channel $\left\{ \begin{smallmatrix} S'BCT \\ S'QT \end{smallmatrix} \right\}$ between S' and T . By the well-known principles of electric conduction, we have

$$R = \frac{1}{\frac{1}{S'BCT} + \frac{1}{S'QT}} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

for the resistance in the double arc between S' and T . Then, by addition, we have

$$SS' + R + TT',$$

for the resistance from S to T' by the channel $SS' \left\{ \begin{smallmatrix} S'BCT \\ S'QT \end{smallmatrix} \right\} TT'$.

This whole resistance is divided, by Q and its equipotential point in the direct channel $S'BCT$, into the parts

$$SS' + \frac{S'Q}{S'QT} \cdot R, \text{ and } \frac{QT}{S'QT} \cdot R + TT'.$$

Hence if, for simplicity, we suppose the potential at S to be 0, and at T' to be E , and if we denote by q the potential at Q , we have

$$q = E \frac{SS' + \frac{S'Q}{S'QT} \cdot R}{SS' + R + TT'} \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Again, since P divides the resistance between S and T' , along the channel SPT' , into the parts SP and PT' , we have

$$p = E \frac{SP}{T'} \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

if we denote by p the potential at P . Hence

$$q - p = E \frac{SS' + \frac{S'Q}{S'QT} \cdot R - \frac{SP}{SPT'} (SS' + R + TT')}{SS + R + TT'};$$

or, since $1 - \frac{SP}{SPT'} = \frac{PT'}{SPT'}$,

$$q - p = E \frac{\frac{PT'}{SPT'} \cdot SS' - \frac{SP}{SPT'} \cdot TT' + R \left(\frac{S'Q}{S'QT} - \frac{SP}{SPT'} \right)}{SS + R + TT'} \quad . \quad . \quad (4)$$

Now let us suppose that, by varying one or more of the component arcs in the balance-circuit, we reduce the galvanometer indication to zero, that is to say, make $q - p = 0$. We shall have by equating the numerator of the preceding expression to zero, and resolving for TT' ,

$$TT' = \frac{PT'}{SP} \cdot SS' + R \left(\frac{SPT'}{S'QT} \frac{S'Q}{SP} - 1 \right) \quad . \quad . \quad . \quad (5)$$

To interpret this expression, it may be remarked that if the second

term vanishes, that is to say, if

we have

$$\left. \begin{aligned} R\left(\frac{SPT'}{S'QT} \cdot \frac{S'Q}{SP} - 1\right) &= 0 \\ TT' &= \frac{PT'}{SP} \cdot SS' \end{aligned} \right\}; \dots \dots \dots (6)$$

and this is the condition aimed at in the arrangement. Now the connexions at S and T' must be made so good that the resistance SG in the first is inappreciable in comparison with GP, and the resistance HT', in the second, inappreciable in comparison with PH; so that we may have

$$\frac{PT'}{SP} \approx \frac{PH}{GP},$$

where \approx denotes an equality not perfect, but having no appreciable error: and hence

$$TT' \approx \frac{PH}{GP} \cdot SS'.$$

The condition

$$R\left(\frac{SPT'}{S'QT} \cdot \frac{S'Q}{SP} - 1\right) \approx 0$$

is to be secured by one or other of two ways or by both combined; that is, by making

$$R \approx 0 \dots \dots \dots (a)$$

or

$$\frac{SPT'}{S'QT} \cdot \frac{S'Q}{SP} \approx 1, \dots \dots \dots (b)$$

or each as nearly as possible. If the connexion BC were quite perfect and the marks S' and T were at the very ends of the conductors, the condition (a) would be fulfilled and there would be no necessity for the condition (b). We should then have a perfect Wheatstone balance,—the secondary testing-conductor $\left\{ \begin{smallmatrix} K \\ L \end{smallmatrix} \right\} Q$ becoming merely a part of the galvanometer electrode. Hence whenever the resistance S'T can be made absolutely insensible, Wheatstone's balance leaves nothing to desire, provided the ends of the testing-conductor are applied to marked points on the standard and tested conductors, and the battery electrodes to their outer ends, or to points of them between their outer ends and those marked points. When, however, as very frequently is the case, S'T may be made small but not absolutely insensible in comparison with the resistances of the standard and tested conductors, the addition of the "secondary testing-conductor" becomes valuable, even if it be only arranged to give a rough approximation to the condition $\frac{SPT'}{S'QT} \cdot \frac{S'Q}{SP} = 1^*$, since it will

* This of course is equivalent to $SPT' : SP :: S'QT : S'Q$, and means that the secondary conductor is to be divided by one galvanometer electrode in the same proportion as the primary is divided by the other.

reduce the error to the fraction $\frac{SPT'}{S'QT} \cdot \frac{S'Q}{SP} - 1$, of the small resistance R^* . But further, when, as in experiments on short thick bars like those of Mr. Calvert, $S'T$ cannot by any management be got to be small in comparison with TT' , the use of the secondary testing-conductor becomes essential, and the most accurate possible fulfilment of the condition

$$\frac{SPT'}{S'QT} \cdot \frac{S'Q}{SP} = 1$$

must be aimed at. This is to be done by dividing the secondary testing-conductor at Q , in very exactly the same ratio as the primary at P , and taking care that the resistances in the connexions $S'K$, LT are very small in comparison with KQ and QL .

Part II. Suggestions for carrying out these principles in practice.

When high accuracy is not required, the two “testing-conductors” may be made of wires stretched straight in parallel lines, and the connexions for the galvanometer electrodes may be applied to them by means of a slide on a graduated scale—as in one of the common forms of Wheatstone’s balance, with sliding contact on single testing-conductor. This form is very objectionable, however, whether for Wheatstone’s balance or the method I now propose: (1) because it is impossible to secure that the different parts of each testing-conductor shall be accurately at the same temperature; (2) because the resistances at the ends of the fine stretched wire or wires are always sensible in comparison with the smallest measured differences produced by the slide; (3) because the stretched wire itself is never of absolutely equal gauge throughout, and, even if sensibly so when first put into the instrument, soon ceases to be so in consequence of the friction of the sliding contact which it experiences in use†; (4) because, in even the hastiest experiments, provided a rationally planned galvanometer is used, a far higher proportional degree of accuracy is easily attained in measuring electrical resistances against a standard of resistance than can be at all attained, without very extraordinary precautions and the assistance of a microscope, in measuring lengths under a yard or two against a standard of length.

When the highest accuracy is required, I always use for primary testing-conductor the bisected conductor which I described to the British Association at its Glasgow meeting in 1855. This consists of a fine, very perfectly insulated wire, doubled on itself and wound on a bobbin, with very stout terminals soldered to its ends, and an electrode soldered to its middle, for joining to the galvanometer electrode. The two terminal and the middle electrodes thus attached to the testing-conductor, I have generally hitherto made flexible, either of thick wire, or strand of wires like the conductor of a submarine cable; but, for many applications, it is more convenient

* In such cases R will, according to equation (1) above, be nearly equal to $S'BCT$, but somewhat less.

† This defect I have remedied by frequently putting in a new wire for testing-conductor in working with a sliding-scale Wheatstone’s balance.

to make them solid metal blocks, with binding screws, insulated rigidly upon the bobbin which bears the conductor. The two halves into which the conductor is doubled must be very accurately equalized as to electric resistance when they are wound on the bobbin, and before the terminals are finally attached. This I find can be done with great accuracy; and when, after the terminals are soldered on, the electric bisection is once found perfect, it seems to remain so, without sensible change, for years. The close juxtaposition of the two branches of the testing-conductor on this plan ensures an almost absolute equality of temperature between them in all circumstances, and thus renders easy a degree of accuracy in the measurement of resistances quite unattainable with any other form of Wheatstone's balance. In the new method which I now propose for low resistances, I make the secondary conductor on exactly the same plan, and generally of about the same dimensions, as the primary. The bisected testing-conductors are only available when the resistances of the standard and of the tested conductor can be made equal; and with them the method which has been described above seems to be the most accurate possible for testing a perfect equality of resistance between two conductors.

The same plan of testing-conductors seems still the best, even when testing by equality cannot be practised,—with only this difference, that the two branches of each testing-conductor, instead of being made of equal resistance, must be adjusted to bear to one another very exactly the ratio which the tested resistance is to bear to the standard. By proper care, to prevent the bobbin of either testing-conductor from getting any non-uniform distribution of temperature, great accuracy may still be secured; but it is scarcely possible to maintain so very close an agreement of temperature, and therefore so constant a ratio of resistances, as when the two branches are equal lengths of one wire coiled side by side.

The use of this plan of conductors divided in a fixed ratio, whether for the single testing-conductor in Wheatstone's balance, or for the primary and secondary testing-conductors in the new method now proposed, requires that either the standard or the tested conductor can be varied so as to adjust the resistance of one to bear precisely that ratio to the resistance of the other. In certain cases this may be done advantageously by shifting one or other of the contacts S, S', T, T' along the standard or the tested conductor, as the case may be. If, for instance, T or T' can be shifted conveniently, the object of the measurement may be to find by trial on the tested conductor a portion TT' from mark to mark, of which the resistance bears a stated ratio to the fixed standard SS' from mark to mark. But by far the easiest working, and in most cases the most accurate also, is to be done by means of a well-arranged series of standards with terminals adapted for combining them in such a manner as to give to a minute degree of accuracy whatever resistance may be required. In a future communication on standards of electric resistance, I intend to describe plans for attaining this object through a wide range of magnitude (resistances from 10^5 to 10^{13} British absolute

units of feet per second on Weber's invaluable system). In the mean time I shall merely say that I have formed a plan which I expect will prove very advantageous for low resistances, and which consists in combining the standards, whichever of them are required, in multiple arc (or "parallel" arcs, according to an expression sometimes used), so as to add their *conducting powers**,—instead of in series, as in all arrangements of resistance coils hitherto used, by which the *resistances* of the component standards are added.

Part III. General Remarks on Testing by Electro-dynamic Balance.

I shall conclude by remarking that the sensibility of the method which has been explained, as well as of Wheatstone's balance, is limited solely by the heating effect of the current used for testing. To estimate the amount of this heating effect, let e and f be the parts of the whole electromotive force, E , which act in the standard SS' , and tested conductor TT' respectively; so that, in accordance with the notation used above, we have

$$\left. \begin{aligned} e &= E \frac{SS'}{SS' + R + TT'} \\ f &= E \frac{TT'}{SS' + R + TT'} \end{aligned} \right\} \dots \dots \dots (7)$$

of its substance. Following Weber, I define the resistance of a bar or wire one foot long, and weighing one grain, its *specific resistance*. It is much to be desired that the *weight-measure*, rather than the *diameter* or the *volume-measure*, should be generally adopted for accurately specifying the gauge of wires used as electric conductors.

With reference to either SS' or TT' (the first, for instance), let us use the following notation :—

- l its length in feet ;
- w its mass per foot in grains ;
- s the specific heat of its substance ;
- σ the specific resistance of its substance.

Thus, since we have taken SS' to denote its actual resistance, we have

$$SS' = \frac{\sigma l}{w}.$$

Now, Weber's system of absolute measurement for electromotive forces and for resistances being followed, I have shown† that *mechanical value of the heat generated per unit of time in a fixed conductor of uniform metallic substance is equal to the square of the electromotive force between its extremities, divided by its resistance*. This in the present case is equal to

$$\frac{e^2 w}{l \sigma};$$

* The reciprocal of the resistance of a "conductor" or "arc" I call its *conducting power*. The conducting power of a bar or wire of any substance one foot long and weighing one grain, I call the *specific conductivity of its substance*.

† In a paper "On the Mechanical Theory of Electrolysis," Philosophical Magazine, Dec. 1851.

and if J denote Joule's mechanical equivalent of the thermal unit, we therefore have

$$\frac{e^2 w}{J l \sigma}$$

for the rate per second at which heat is generated in SS' . This will at first go entirely to raise its temperature*. Now wl is its mass in grains, and therefore wls is its whole thermal capacity; and if we divide the preceding expression by this, we find

$$\frac{e^2}{J l^2 s \sigma}$$

for the rate per second at which it commences to rise in temperature at the instant when the battery is applied. If we call $\frac{e}{l}$ the electromotive force per foot, we may enunciate the result thus:

The rate at which a linear conductor of uniform metallic substance commences rising in temperature at the instant when an electric current commences passing through it, is equal to the square of the electromotive force per unit of length divided by the continued product of Joule's equivalent into the specific heat of the substance, into the specific resistance of the substance.

Let us suppose, for example, that the conductor in question is copper of best electric conductivity. Its specific resistance will be about 7×10^6 , and its specific heat about $\cdot 1$. The value we must use for Joule's equivalent will be $32\cdot 2$ times the number 1390, which Joule found for the mechanical value in foot-grains of the thermal unit Centigrade, since the absolute unit of force, being that force which acting on a grain of matter during a second of time generates 1 foot per second of velocity, is $\frac{1}{32\cdot 2}$ of the weight of a grain in middle latitudes of Great Britain. Thus we find

$$J = 44758.$$

Hence the expression for the rate in degrees Cent. per second, at which the temperature begins rising in a copper conductor, is

$$\frac{\left(\frac{e}{l}\right)^2}{313 \times 10^8}.$$

So, I have found the electromotive force of a single cell of Daniell's to be about $2\cdot 3 \times 10^6$ British absolute units†; and if we suppose $\frac{1}{n}$ of this to go to each foot of the conductor in question, we shall have

$$\left(\frac{e}{l}\right) = \frac{2\cdot 3^2 \times 10^{12}}{n^2} = \frac{5\cdot 29 \times 10^{12}}{n^2};$$

* As soon as it has risen sensibly in temperature it will begin to give out heat by conduction, or by conduction and radiation, to the surrounding matter; and the rate at which it will go on rising in temperature will be the rate expressed by the formula in the text (with the true specific resistance, &c., for each temperature), diminished by the rate of loss to the surrounding matter.

† Proceedings of the Royal Society, February 1860.

and therefore the expression for the rate of heating becomes

$$1.69 \times \frac{100}{n^2}.$$

Now, by using a sufficiently large single cell, we may make the electromotive force, E , between S and T' , be as little short as we please of the whole electromotive force of the cell. We might then, in testing by equality, with a standard and a tested conductor each three inches or so long, and using a single cell, have nearly as much as half the electromotive force of one cell acting per quarter foot of these conductors, or two cells per foot. Hence if either is of best conductive copper, its temperature would commence rising at the rate of $4 \times 169^\circ$ or 676° Cent. per second. It would be almost impossible to work with so high a heating effect as this. But if we use only $\frac{1}{10}$ th of the supposed electromotive force, that is to say $\frac{1}{5}$ th of a cell per foot of the copper conductor, the rate of heating will be reduced to $\frac{1}{100}$, that is to say, will be 6.76 per second. By using only very brief battery applications, it would be possible to work with so high a rate of heating as that, without having the results much vitiated by it. But $\frac{1}{50}$ of a cell per foot will give only $.0676^\circ$ of heating effect per second, and will be quite a sufficient battery power to use in most cases. In the case we have supposed, for instance, of conductors only three inches long, the electromotive force on each would then be about $\frac{1}{200}$ of the electromotive force of the cell. What we denoted above by e and f in equations (7) would therefore each have this value. Hence, by equation (4), we see that the effect of a difference of $\frac{1}{1000}$ between SS' and TT' would be to give $q-p$ the value $\frac{1}{400000}$ of the electromotive force of a single cell. Now one of the light mirror* galvanometers, which I commonly use, reflecting the image of a gas or paraffine lamp to a scale 25 inches distant, would, if made with a coil of 50 yards of copper wire of moderate quality, weighing 5 grains per foot, give a deflection of half a division of $\frac{1}{40}$ of an inch on this scale, with an electromotive force of $\frac{1}{400000}$ of a single cell†. Hence by using such a galvano-

* The mirror is a circle of thin "microscope glass" about three-eighths of an inch in diameter, silvered in the ordinary manner; and a small piece of flat file steel of equal length, attached to its back by lac varnish, constitutes the "needle" of the galvanometer. The whole weight of mirror and needle amounts to from 1 to $1\frac{1}{2}$ grain. It is suspended inside the galvanometer coil by single silk fibre about $\frac{1}{8}$ inch long. It is necessary to try many mirrors thus prepared, each with its magnet attached, before one is found giving a good enough image. I am much indebted to Mr. White, optician, Glasgow, for the skill and patience which he has applied to the very troublesome processes involved.

† In this state of sensibility the needle is under Glasgow horizontal magnetic force of the earth alone; and, with its mirror, it makes a vibration one way in about .7 of a second. In many uses of my form of mirror galvanometer, both for telegraphic and for experimental purposes, I find it convenient to make its indications still more rapid, though, of course, less sensitive, by increasing the directive force by means of fixed steel magnets. On the other hand, I use fixed steel magnets to diminish the earth's directing force and make the needle more sensitive, when very high sensibility is wanted; but this would be inconvenient for the application described in the text, because effects of thermo-electric action would be made too prominent.

meter, and primary and secondary conductors of sufficient resistances to fulfil the condition of doing away with sensible error from imperfect connexions in the manner explained above, but yet of resistances either less than or not many times greater than the resistance of the galvanometer coil, it is easy to test to $\frac{1}{1000}$ the resistance of a copper wire or bar not more than 3 inches long. The current we have found to be sufficient for this object would only produce a heating effect of $\cdot 14^{\circ}$ in two seconds, which, with good apparatus, is more than enough of time, as I shall show presently. The influence of this heating effect may be regarded as nearly insensible, since even as much as $\cdot 2^{\circ}$ only alters the resistance of copper by about $\frac{3}{4000}$.

In all measurements of electric resistance, whatever degree of galvanic power is used, a spring "make and break" key* ought to be placed in one of the battery electrodes, so that the current may never flow except as long as the operator wills to keep it flowing, and presses the key. I introduce a second similar spring key in one of the galvanometer electrodes (that is between either Q or P and the galvanometer coil), so arranged that the pressure of the operator's finger on a little block of vulcanite attached to either spring shall first make the contact of the first spring (completing the battery circuit), and when pushed a little further, shall make the contact of the second spring and complete the galvanometer circuit. The test for the balance of resistances will then be that not the slightest motion of the needle is observable as a consequence of this action on the part of the operator. The sensibility of the arrangement is doubled by a convenient reverser in the galvanometer circuit, by which the current, if any, may be reversed easily by the operator while keeping the two connexions made by full pressure on the double spring key just described. Another convenient reverser should be introduced into the battery circuit, to eliminate effects of thermo-electric action if sensible.

It may often happen, unless the galvanometer is at an inconveniently great distance from the conductors tested, that its needle will be directly affected to a sensible extent by the main testing-current; but with the arrangement I have proposed the observer tests whether or not this is the case by pressing the double spring-key to only its middle position (battery contact alone made), and watching whether or not the needle moves perceptibly. If it does not move perceptibly, he has nothing more to do than immediately to press the double key home, to test the balance of resistances. If the needle does move when the key is pressed to its middle position, he may, when in other respects allowable, keep the current flowing by holding the key in its middle position till the needle comes to rest, or at least till it shows the point towards which its oscillations converge, and then press home to test the balance of resistances. When the very highest accuracy is aimed at, or when, for any reason (as, for

* Morse's original telegraph key, which instrument-makers have "improved" into the in every respect worse form in which it is now commonly made—a massive contact-lever urged by a spring.

instance, extreme shortness in the standard or tested conductor), only the shortest possible duration of current is allowable, the position of the galvanometer, with reference to the battery and the other portions of circuit, must be so arranged that its needle may show no sensible deflection when the key is pressed to the middle position. Ignorant or inadvertent operators are probably often led into considerable mistakes in their measurements of resistance by confounding deflections due to direct electro-magnetic influence of battery, battery electrodes, or standard, tested, or testing-conductors, on the needle of the galvanometer, with the proper influence of a current through its own coil,—a confusion which can only be resolved by making or breaking the galvanometer circuit while the battery circuit is kept made, for which there is no provision in the ordinary plans of Wheatstone's balance. We may, however, suppose that most experimenters will be sufficiently upon their guard against error from such a source. But there is another and a much more important advantage in the double-break arrangement which I now propose. Electro-magnetic inductions will generally be sensible* in some or in all of the different branches of the compound circuit, and cannot, except in very special cases, be exactly balanced as regards electromotive force between P and Q with the arrangement which makes an exact balance of resistances. Hence, at the moment when the battery contact is made, there must generally be an electromotive impulse between Q and P, which will drive a current through the galvanometer coil, and make an embarrassing deflection of the needle if the galvanometer circuit is complete at that instant (as it is in the common plans of Wheatstone's balance), and will require the observer to wait until the needle comes to rest, or until he can tell precisely to what point its oscillations converge, the current being kept flowing all the time, before he can discover whether the balance of resistances has been attained or not. This absolutely precludes very refined testing, since, whether by the heating and consequent augmentation of resistance of some part of the balanced branches, or by thermo-electric reactions consequent on heating and cooling effects at junctions of dissimilar metals when the branches of the balance are not all of one homogeneous metal, or last, though not least, by the eye losing the precise position where the galvanometer needle or indicating image rested, it is not possible to use the full sensibility of the galvanometer for testing a zero if its needle is allowed to receive such a shock in the course of the weighing. Embarrassment from this source is completely done away with by using the double spring key described above, and giving time, from its first to its second contact, to allow the electro-magnetic induction to subside. An extremely small fraction of a second is enough in almost all cases; and the operator may therefore generally press the key home almost

* I make them as little sensible as possible in my coiled testing-conductors, and in sets of coiled standards of resistance, by either doubling each coil or each branch of each coil on itself, or by reversing the lathe at regular intervals in winding on any single coil on a bobbin,—a plan which has also the advantage of rendering the direct electro-magnetic action of any coil so wound very small or quite insensible on any galvanometer needle in its neighbourhood.

as sharply as he will or can. But when there is a large “electrodynamic capacity”* in any part of the balance-circuit, as, for example, when the coil of a powerful electro-magnet with soft iron† core is the conductor whose resistance is tested, it may be necessary to keep the key in its middle position for a few seconds before pressing it home, to avoid obtaining what might be falsely taken for an indication of too great a resistance to conduction (or “frictional” resistance, as I have elsewhere called it‡), being a true indication of resistance or reaction of inertia to the commencement of the current in the electro-magnetically-loaded branch§. In such cases it is impossible, either by electrodynamic balance or in any other way, to obtain a measurement of resistance without keeping the battery applied for the few seconds required to produce sensibly its final strength of current undiminished by inductive reaction, over and above the time required to get an indication from the galvanometer. But, as already remarked, in all ordinary cases, the inductive reaction becomes insensible after a very small fraction of a second, and the operator may press the double key home to its second contact almost as sharply as he pleases. With such a galvanometer as I have described, he need not hold it down for more than .7 of a second (the time of the simple vibration of the needle||) to test the balance of resistances. The order of procedure will therefore generally be this:—The operator will first strike the key sharply, allowing it to rise again instantly, adjust resistances in the balance-circuit according to

* This term I first introduced in a communication “On Transient Electric Currents” (Phil. Mag., June 1853), to designate what for any electric current through a given conductor is *identical* in meaning with the “simple-mass equivalent” in the motion of Attwood’s machine as ordinarily treated. A rule for calculating the electrodynamic capacity is given in that communication; also the rule, with an example, in Nichol’s Cyclopædia, article “Magnetism—Dynamical Relations of.”

† Giving a resistance to the commencing, to the ceasing, or to any other variation in the strength of an electric current (precisely analogous to the effect of inertia on a current of common fluid),—which it seems quite certain must be owing to true inertia (not of what we should at present regard as the electric fluid or matter itself flowing through the conductor, but) of motions accompanying the current, chiefly rotatory with axes coinciding with the lines of magnetic force in the iron, air, and other matter in the neighbourhood of the conductor, and continuing unchanged as long as the current is kept unchanged. See Nichol’s Cyclopædia, article “Magnetism—Dynamical Relations of,” edition 1860; also Proceedings of the Royal Society, June 1856; or Phil. Mag., vol. Jan.–June 1857.

‡ “Dynamical Theory of Heat, Part VI., Thermo-electric Currents,” Transactions of the Royal Society of Edinburgh, 1854; and Phil. Mag. 1856.

§ It is probable that a Wheatstone’s balance, perfectly adjusted for equilibrium of resistances to conduction, and used with the galvanometer circuit constantly made, so as to show the whole effect of the inductive impulse, may afford the best means for making accurate metrical investigations on electro-magnetic induction, and especially for determining “electrodynamic capacities” in absolute measure.

|| The mirror galvanometers commonly used in Germany have all much longer periods (ten or twenty times as long in many cases) for the vibration of their needles, and want proportionately longer contacts to obtain full advantage of their sensibility,—in each case a contact during a time equal to that of the vibration of the needle one way being required for this purpose.

the indication of the galvanometer; strike the key sharply again, readjust resistances; and so on, until the balance is nearly attained. He will go on repeating the process, but holding the key down rather longer each time. At the last he will press the key gently down, hold it pressed firmly for something less than a second of time, and let it rise again; and if the spot of light reflected from the mirror of the galvanometer does not move sensibly, the resistances are as accurately balanced as he can get them.

ROYAL INSTITUTION OF GREAT BRITAIN.

June 20, 1862.—The Duke of Northumberland, K.G., F.R.S.,
President, in the Chair.

“On Gas Furnaces,” &c. By M. Faraday, Esq., D.C.L., LL.D., F.R.S., Fullerian Professor of Chemistry, Royal Institution.

The subject of the evening was gas glass-furnaces; and having arisen almost extemporaneously, it resolved itself chiefly into an account of the manner in which Mr. Siemens has largely and practically applied gas, combined with the use of his heat-regenerator, to the ignition of all kinds of great furnaces. Gas has been used to supply heat, even upon a very large scale, in some of the iron-blast-furnaces; and heat which has done work once has been carried back in part to the place from whence it came to repeat its service; but Mr. Siemens has combined these two points, and successfully applied them in a great variety of cases—as the potter’s kiln, the enameller’s furnace, the zinc-distilling furnace, the tube-welding furnace, the metal-melting furnace, the iron-puddling furnace, and the glass-furnace either for covered or open pots—so as to obtain the highest heat required over any extent of space, with great facility of management, and with great economy (one-half) of fuel. The glass-furnace described had an area of 28 feet long and 14 feet wide, and contained eight open pots, each holding nearly two tons of material.

The gaseous fuel is obtained by the mutual action of coal, air, and water at a moderate red heat. A brick chamber, perhaps 6 feet by 12, and about 10 feet high, has one of its end walls converted into a fire-grate; *i. e.* about halfway down it is a solid plate, and for the rest of the distance consists of strong horizontal plate-bars where air enters, the whole being at an inclination such as that which the side of a heap of coals would naturally take. Coals are poured, through openings above, upon this combination of wall and grate, and being fired at the under surface, they burn at the place where the air enters; but as the layer of coal is from 2 to 3 feet thick, various operations go on in those parts of the fuel which cannot burn for want of air. Thus the upper and cooler part of the coal produces a larger body of hydrocarbons; the cinders or coke which are not volatilized approach, in descending, towards the grate; that part which is nearest the grate burns with the entering air into carbonic acid, and the heat evolved ignites the mass above it; the carbonic acid, passing slowly through the ignited carbon, becomes converted into carbonic oxide, and mingles in the upper part of the chamber (or gas-producer) with the former hydrocarbons. The water, which is purposely introduced

at the bottom of the arrangement, is first vaporized by the heat, and then decomposed by the ignited fuel and rearranged as hydrogen and carbonic oxide; and only the ashes of the coal are removed as solid matter from the chamber at the bottom of the fire-bars.

These mixed gases form the gaseous fuel. The nitrogen which entered with the air at the grate is mingled with them, constituting about a third of the whole volume. The gas rises up a large vertical tube for 12 or 15 feet, after which it proceeds horizontally for any required distance, and then descends to the heat-regenerator, through which it passes before it enters the furnaces. A regenerator is a chamber packed with fire-bricks, separated so as to allow of the free passage of air or gas between them. There are four placed under a furnace. The gas ascends through one of these chambers, whilst air ascends through the neighbouring chamber, and both are conducted through passage outlets at one end of the furnace, where mingling they burn, producing the heat due to their chemical action. Passing onwards to the other end of the furnace, they (*i. e.* the combined gases) find precisely similar outlets down which they pass; and traversing the two remaining regenerators from above downwards, heat them intensely, especially the upper part, and so travel on in their cooled state to the shaft or chimney. Now the passages between the four regenerators and the gas and air are supplied with valves and deflecting-plates, some of which are like four-way cocks in their action; so that by the use of a lever these regenerators and air-ways, which were carrying off the expended fuel, can in a moment be used for conducting air and gas into the furnace; and those which just before had served to carry air and gas into the furnace, now take the burnt fuel away to the stack. It is to be observed that the intensely-heated flame which leaves the furnace for the stack always proceeds downwards through the regenerators; so that the upper part of them is most intensely ignited, keeping back, as it does, the intense heat; and so effectual are they in this action, that the gas which enters the stack to be cast into the air is not usually above 300° F. of heat. On the other hand, the entering gas and air always pass upwards through the regenerator; so that they attain a temperature equal to white heat before they meet in the furnace, and there add to the carried heat that due to their mutual chemical action. It is considered that, when the furnace is in full order, the heat carried forward to be evolved by the chemical action of combustion is about 4000° , whilst that carried back by the regenerators is about 3000° , making an intensity of power which, unless moderated on purpose, would fuse furnace and all exposed to its action.

Thus the regenerators are alternately heated and cooled by the outgoing and entering gas and air; and the time for the alternation is from half an hour to an hour, as observation may indicate. The motive power on the gas is of two kinds—a slight excess of pressure within is kept up from the gas-producer to the bottom of the regenerator to prevent air entering and mingling with the fuel before it is burnt; but from the furnace, downwards through the regenerators, the advance of the heated medium is governed mainly by the draught in the tall stack, or chimney.

Great facility is afforded in the management of these furnaces. If, whilst glass is in the course of manufacture, an intense heat is required, an abundant supply of gas and air is given; when the glass is made, and the condition has to be reduced to working-temperature, the quantity of fuel and air is reduced. If the combustion in the furnace is required to be gradual from end to end, the inlets of air and gas are placed more or less apart the one from the other. The gas is lighter than the air; and if a rapid evolution of heat is required, as in a short puddling-furnace, the mouth of the gas inlet is placed below that of the air inlet; if the reverse is required, as in the long tube-welding furnace, the contrary arrangement is used. Sometimes, as in the enameller's furnace, which is a long muffle, it is requisite that the heat be greater at the door end of the muffle and furnace, because the goods, being put in and taken out at the same end, those which enter last and are withdrawn first remain, of course, for a shorter time in the heat at that end; and though the fuel and air enters first at one end and then at the other alternately, still the necessary difference of temperature is preserved by the adjustment of the apertures at those ends.

Not merely can the supply of gas and air to the furnace be governed by valves in the passages, but the very manufacture of the gas fuel itself can be diminished, or even stopped, by cutting off the supply of air to the grate of the gas-producer; and this is important, inasmuch as there is no gasometer to receive and preserve the aëri-form fuel, for it proceeds at once to the furnaces.

Some of the furnaces have their contents open to the fuel and combustion, as in the puddling and metal-melting arrangements; others are enclosed, as in the muffle furnaces and the flint-glass-furnaces. Because of the great cleanliness of the fuel, some of the glass-furnaces, which before had closed pots, now have them open, with great advantage to the working, and no detriment to the colour.

The economy in the fuel is esteemed practically as one-half, even when the same kind of coal is used either directly for the furnace or for the gas-producer; but as in the latter case the most worthless kind can be employed, such as slack, &c., which can be converted into a clean gaseous fuel at a distance from the place of the furnace, so many advantages seem to present themselves in this part of the arrangement.

It will be seen that the system depends, in a great measure, upon the intermediate production of carbonic oxide from coal, instead of the direct production of carbonic acid. Now carbonic oxide is poisonous, and, indeed, both these gases are very deleterious. Carbonic acid must at last go into the atmosphere; but the carbonic oxide ceases to exist at the furnace, its time is short, and whilst existing it is confined on its way from the gas-producer to the furnace, where it becomes carbonic acid. No signs of harm from it have occurred, although its application has been made in thirty furnaces or more.

The following are some numbers that were used to convey general impressions to the audience. Carbon, burnt perfectly into carbonic acid in a gas-producer, would evolve about 4000° of heat, but if burnt into carbonic oxide it would evolve only 1200° . The carbonic

oxide, in its fuel form, carries on with it the 2800° in chemical force, which it evolves when burning in the real furnace with a sufficient supply of air. The remaining 1200° are employed in the gas-producer in distilling hydrocarbons, decomposing water, &c. The whole mixed gaseous fuel can evolve about 4000° in the furnace, to which the regenerator can return about 3000° more.

GEOLOGICAL SOCIETY.

[Continued from p. 76.]

May 21, 1862.—Poof. A. C. Ramsay, President, in the Chair.

The following communications were read:—

1. "On the Metamorphic Rocks of the Banffshire Coast, the Scarabins, and a portion of East Sutherland." By Prof. R. Harkness, F.R.S., F.G.S.

The coast-section from Gamrie to Buckie was first described; it consists mainly of folded gneiss and grauwacke sandstone and shale, with underlying quartz-rock, of great thickness, conformable, and folded with it, and seen in anticlinals at Melross, Banff, and Duni-dich, and still more in an arch between Cullen and Buckie. Two folds of limestone, obscurely stratified, and not persistent, occur with the schists at the Burn of Boyne and Dunidich. The dykes of syenite, of granite, and of serpentine (Portsoy) were also pointed out in this section, as well as two outliers of the Old Red deposits at Dunidich and Cullen. The metamorphic rocks above mentioned have a predominating south-east dip, and the folds hang over to the north-west; but the author regards these strata as holding a reversed position, the gneissose and grauwacke strata being really the uppermost of the series, as in other parts of the North of Scotland.

The section from the sea at Berridale, across the Scarabins, to Strath-na-ver was next described. Here the granite of Bean-na-aiglesh succeeds to the Old Red Sandstone of Berridale, and is succeeded by the gneiss and folded white quartz-rock of the Scarabins. From the Scarabins to Strath-na-ver granite and gneiss alternate in laminar masses, dipping south-east, towards the Scarabins, here and there bearing unconformable outliers of Old Red Sandstone. In this case also the author pointed out that a reversed dip obtained, by which the really uppermost gneissose rock was made to appear lower in position than the quartzite.

Professor Harkness further alluded to the conformability of the granite with the strata in this district, and to the probability of its being rather the result of an excessive amount of metamorphic action than of plutonic origin.

2. "On the Geology of the Gold-fields of Nova Scotia." By the Rev. David Honeyman.

The author, at the request of the Provincial Government Commission for the International Exhibition, made some observations on the auriferous rocks at Allen's and Laidlow's farms, near the junction of the Halifax-and-Windsor and the Halifax-and-Truro Railways. He found chloritic schist, with vertical auriferous quartz-veins, and

a gold-bearing horizontal quartz-vein (the "barrels" of the miners) lying on the schist and overlain by quartzite and gravel. By the neighbouring railway-sections the chlorite-schist is seen to alternate in broad bands with quartzite, and to be associated with granite. The author thinks there is reason to believe that the quartzite may be of Lower Silurian age.

3. "On some Fossil Crustacea from the Coal-measures and Devonian Rocks of New Brunswick, Nova Scotia, and Cape Breton." By J. W. Salter, Esq., F.G.S., of the Geol. Surv. Great Britain.

One of the Devonian fossils is apparently allied to the Stomapods, and is named *Amphipeltis paradoxus* by Mr. Salter; it was obtained by Dr. Dawson near St. John's, where it occurred with plant-remains; another Crustacean fossil from the same locality is a new *Eurypterus*, *E. pulicaris*. Other remains of *Eurypteri* have been sent also by Dr. Dawson, from the Coal-measures of Port Hood and the Joggins; and with these a new Amphipod, *Diplostylus*, having some characters of alliance with *Typhis* and *Brachyocelus*.

4. "On some Species of *Eurypterus* and allied forms." By J. W. Salter, Esq., F.G.S., &c.

After alluding to the late and complete researches on *Eurypterus* by Dr. Wieskowski and Professor J. Hall, Mr. Salter explained some formerly obscure points in its structure, and proceeded to describe the *E. Scouleri*, Hibbert, from the Carboniferous limestone of Scotland, and the Upper Old Red Sandstone of Kilkenny; the *E. (Arthropleura) mammatus*, sp. nov., from the Upper Coal-measures near Manchester; and *E. ? (Arthropleura ?) ferox*, sp. nov., from the Coal-measures of North Staffordshire.

5. "On *Peltocaris*, a new genus of Silurian Crustacea." By J. W. Salter, Esq., F.G.S., &c.

Of this form an imperfect individual, from the anthracite-shales (Llandeilo flags) of Dumfriesshire, was formerly described by the author as *Dithyrocaris ? aptychoides*. Better specimens enabled him to distinguish it as a new generic form belonging to the Phyllopods, not far removed from *Hymenocaris* and *Dithyrocaris*. A fragment of another larger form, from the same locality, is described by the author as *Peltocaris ? Harknessi*. Mr. Salter also explained his views of the relationship of the palæozoic *Phyllopoda*, among themselves and with the recent forms, and illustrated them by a diagram in which they were arranged in chronological succession.

6. "On a Crustacean Track in the Llandeilo Flags of Chirbury, Shropshire." By J. W. Salter, Esq., F.G.S., &c.

This track consists of numerous, short, narrow, oblique, chisel-shaped imprints, on the ripple-ridges of the slab; and, according to the author, it must have been caused by a small Crustacean with a bifid telson or prong-like tail. To a like agency Mr. Salter refers similar markings described by M. Brébisson as occurring in the Lower Silurian sandstone of Noron in the Falaise (Normandy).

XXII. *Intelligence and Miscellaneous Articles.*

ON THE DENSITY OF ICE. BY L. DUFOUR.

IN a previous communication* the author determined the density of ice by making a mixture of alcohol and water of exactly the same density as the ice, so that the ice was everywhere in equilibrium in it, and then determining the density of this mixture. This method, however, laboured under the objection that the alcoholic mixture dissolved some of the ice, which vitiated the result.

In a recent series of determinations he has used a mixture of chloroform and rock-oil for the same purpose. This mixture does not dissolve any ice, it keeps fluid below 0° , and the density of the mixture can be made to vary between 0.82 and 1.50. It is easy to make a mixture so that ice remains in it in perfect equilibrium; if then a little chloroform or a little rock-oil be added, the ice will either rise or sink.

Preliminary trials showed that the mixture retained its homogeneity for a longer time than was necessary for an experiment. In these trials he used the method of hollow glass floaters, the density of which could be determined by ordinary methods. Four trials showed a mean difference of 0.0013 between the density of the floaters obtained directly, and that of the mixture in which they were in equilibrium. This approximation is doubtless too rough, and the method which tolerates it could not be recommended for bodies to which ordinary methods are applicable; but in the case of the special difficulties which ice presents, it will be found that this method is not to be despised. By multiplying the determinations, it might be hoped that the *mean* would not be very far from the truth.

The choice of the ice is difficult. Fragments must be used which are free from air-bubbles, and obtained by means of distilled water; but this absolute elimination of air is very difficult. By causing water, boiled for a long time, to freeze in a kind of barometric chamber, he obtained an opaline ice, quite homogeneous, some fragments of which were admirably adapted for the object intended. Their opalescence was not due to the presence of air, but probably to its structure, or to internal cleavages. Although produced in a space where the tension of the remaining air did not exceed half a millimetre, some small bubbles of ice were here and there perceived, and their presence proved by placing the portion of the fragment which contained them at the bottom of a test-tube full of rock-oil.

The specific gravity of the mixture of chloroform and rock-oil was determined by weighing in it a piece of glass, the weight of which in air and in water was known. The temperature, always below 0° C., varied from $-\frac{1}{2}^{\circ}$ to -8° . All necessary corrections were made to reduce the result to 0° , admitting for the coefficient of the cubical expansion of ice the number 0.000158, given by Plücker and Geissler.

The method gave higher and lower limits, between which the density of ice was certainly comprised; for these limits different values were obtained, the most extreme being 0.9207 and 0.9133. These limits comprise the numbers given for the density by Berzelius, by Plücker, and Brünner: the values given by Heinrich, by Kopp, and by Osann are outside these, and, for the specimens of ice operated

* Phil. Mag. vol. xx. p. 248.

upon, are certainly too high or too low. The mean of the sixteen values obtained in these experiments is 0.9178; the greatest differences are +0.0015 and -0.0012; the mean difference is ± 0.0005 . The number 0.9178 only differs by 0.0002 from that of Plücker and Geissler, and is approximately the same as that of M. Brünner (0.9180). This latter coincidence may inspire some confidence, as Brünner's method was quite different from that which Dufour used.

Assuming 0.9178 as the mean density of ice at 0°, it is easily found that a volume of water in freezing produces 1.0895 volume of ice; or that the expansion at the moment of freezing is $\frac{9}{100}$, or $\frac{1}{11}$ of the volume of the water at 0°.—*Comptes Rendus*, May 19, 1862.

A NEW METAL IN THE NATIVE PLATINUM OF ROGUE RIVER, OREGON. BY C. F. CHANDLER, PROFESSOR OF CHEMISTRY AT UNION COLLEGE.

In examining native platinum from the above locality, more than a year ago, I became convinced of the probable existence of a hitherto unobserved metal. I have deferred publishing my observations, hoping to obtain material for a more complete examination; in this I have thus far been disappointed.

The quantity of platinum examined amounted to only a few grammes. It was digested with hydrochloric acid to remove impurities, and the solution thus obtained was subjected to the ordinary routine of qualitative analysis.

A brown precipitate was produced by hydrosulphuric acid, which dissolved readily in hydrochloric acid on the addition of a crystal of chlorate of potassa. In this solution metallic zinc produced a precipitate which resembled metallic tin obtained under similar circumstances. This precipitate dissolved readily in hydrochloric acid on the application of heat; but the solution thus obtained had no effect on a solution of protochloride of mercury (HgCl), and on cooling deposited a small quantity of minute crystals. To guard against error, these experiments were repeated two or three times on small portions of the original solution, always with the same result.

The chloride of this metal differs therefore from the protochloride of tin, in not reducing protochloride of mercury to calomel, and in being but slightly soluble in the cold.

On mentioning my observations to a friend, I was referred to Dr. F. A. Genth's announcement of a new metal, made in 1852*, of which I was not previously aware.

The metal observed by Dr. Genth occurred among grains of platinum from California. It was malleable; it fused readily on charcoal before the blowpipe, becoming covered with a coating of black oxide; it dissolved in borax to a colourless bead, which became opalescent on cooling; it was dissolved by hot hydrochloric acid and by nitric acid; and its solution gave a brown precipitate with hydrosulphuric acid. It seems quite probable, therefore, that the metal which I have observed in the Rogue-River platinum is identical with that observed by Dr. Genth.—*Silliman's American Journ.*, May 1862.

Schenectady, March 6, 1862.

* Proceedings of the Philad. Acad. Nat. Sci., Dec. 1852.

THE
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AND
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[FOURTH SERIES.]

SEPTEMBER 1862.

XXIII. *On the Conformation of the Alps.*

By PROFESSOR TYNDALL, F.R.S.*

DURING the last seven summers I have had opportunities of viewing the Alps from many commanding points of view, and while in such positions have often speculated on the agencies which have given this portion of the earth's surface its remarkable conformation. How have the hills risen, or how have the valleys sunk? I think the mere inspection of the mountains from a sufficient elevation must suggest insuperable difficulties to the assumption that the present mountains have arisen through the action of forces localized beneath their bases, or that the valleys, as they now exist, can have sunk through want of local support underneath. Probably nobody entertains such a notion. Upheaval may have occurred, and sinking may have occurred; but it is next to inconceivable that either action should have been so parcelled out as to produce the present conformation of the Alps. A *general elevation* of the land must be assumed, producing a kind of lopsidedness as regards the figure of the earth; and the question then occurs, how has the land thus elevated been carved into its present form.

In the uplifting of the land, cracks and fissures would probably be produced, and the valleys might be regarded as the traces of these cracks and fissures—widened and deepened, it may be, by subsequent denudation. But the direction of the valleys is not that in which cracks would take place. The valleys generally follow the line of steepest fall, and this would be the line of greatest tension on the lifting of the mass; consequently the mechanical conditions of the problem would lead us to infer cracks at right angles to the present valleys instead of along them. Take, for

* Communicated by the Author.

Phil. Mag. S. 4. Vol. 24. No. 160. Sept. 1862.

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example, the ridge from Monte Rosa over the Lyskamm and Breithorn to the Matterhorn; the upheaval of that ridge could not possibly produce rents in the position now occupied by the Val Tournanche, the Val d'Ayas, and the Val du Lys. The line of strain would be parallel to these valleys, and hence the line of fracture, if fracture at all occurred, across them.

A sufficient consideration of the subject must, I think, result in the conclusion already expressed, that a general elevation of the land formerly existed, and must limit us to the question, "By what agency has this land been scarred so as to exhibit the valleys by which it is now intersected?" These valleys are the tracks of rivers, and have been manifestly formed with reference to the discharge of the aqueous precipitations which occurred on the heights. An eminent Swiss geologist, with whom I had an opportunity of conversing a week or two ago, called the Alpine valleys "*Auswaschungsthäler*," valleys cut out by the action of water. For some years an opposite conclusion has been gradually forcing itself upon me; and this year I completed a chain of evidence which leaves little doubt upon my mind that a mightier excavator than mere water has been at work among the Alps, and that the country owes its present conformation mainly to the action of its ancient glaciers.

It requires some time to realize the stupendous scale on which the ancient ice has operated; and were its traces less indubitable, the judgment would halt before accepting a conclusion involving operations so vast as almost to appear fabulous. This year I walked for the fifth time up the valley of Hasli, observing the action of the ancient glacier upon its boundaries. A million winters may have acted upon these scarred and fluted rocks, and still the scars and the flutings are as distinct as if they had been executed last year. We trace them down to the banks of the Aar, a river which has been rushing for these ages through the valley; and the smallness of its operations must impress us with the *comparative* feebleness of denudation by water. A mighty glacier occupied the valley of the Rhone. I traced it all along the valley to Martigny, a distance of more than 60 miles from the end of the present Rhone glacier. Here, reinforced from Mont Blanc, it ploughed its way towards the Lake of Geneva. Near a station called D'Evionnaz, the *roches moutonnées* rise above each other in heaps, and here, as in Haslithal, the polishing comes down close to the level of the present Rhone,—thus suggesting how *comparatively* small has been the action of the river from the disappearance of the glacier to the present day.

The same thought is continually forced upon the traveller on the south side of the Alps, where the traces of ancient glacier action are, if anything, more astonishing than on the north. Two

years ago I had an opportunity of inspecting the Val Tournanche from Breuil to Chatillon. This year I crossed the Cimes Blanches from Breuil, wandered over the ancient névés of the place, penetrated the higher ramifications of the Val d'Ayas, and everywhere found the sculpture of the ancient ice. Crossing the Col Betta Furka, and looking down the Val du Lys, the work of the glacier which once proceeded from the slopes of the Lyskamm was immediately manifest. I traced the action through this noble valley, as far as Gressonay St. Jean; everywhere on the valleys' flanks the same truth was proclaimed. What air and water have accomplished since the disappearance of the glaciers are mere scratches of the tooth of time, in comparison with the mighty furrows which had been previously ploughed out.

From Gressonay I crossed the Col de Val d'Obbia to Alagna, and everywhere I trod upon ground deserted by the ice. Like its neighbour of the Lys, the Val Sesia also formed the bed of a mighty glacier. From Alagna I crossed the Turloz; the traces of the ice are here magnificent, and on the flank of the Val Anzasca, opposite the place where the glacier from Mont Turloz joined that from Macugnaga, the rocks are wonderfully worn; all up the Macugnaga valley to the base of Monte Rosa the same grand evidences appear, forcing upon the mind the existence of a state of things so extraordinary that, were the proofs less strong, the imagination would shrink from any attempt to realize it. I have repeatedly explored both the Saasthal and Nicolaithal; both of them are glacier channels; an expanse of particularly finely rounded rocks may be seen on the side of the Saasthal at the place where the road turns up to the valley of the Fee.

Having finished the exploration of this part of the country, I felt a strong desire to visit the Jura, and to observe for myself the boulders scattered over its slopes. I accordingly went to Neuchatel, visited the celebrated Pierre-à-Bot, and examined the general character of the moraine-like matter which is strewn over the slopes. Granite boulders were abundant, being in many cases quarried for building use. The Jura, as is well known, yields no granite; it is composed of limestone. Whence, then, did these blocks come? Limestone is a partially soluble rock, and hence, when long exposed to the air, although it may preserve the general form imparted to it by the grinding-action of a glacier, it does not retain the finer striæ which enable one to determine the direction of motion. I therefore sought for slabs of limestone from which the protecting soil had been recently removed, and was fortunate enough to find some very fine ones, in which the glacier-scratches were marked with perfect distinctness. I determined the direction of these scratches at three

different places, and found them coincident: they all pointed to Mont Blanc*, which was the place where the ancient glaciers had received their load, carrying with them across the Canton-de-Vaud, occupying the bed of what is now the Lake of Neuchatel, and depositing them on the slopes of the Jura. The railroad between Neuchatel and Brienne has disclosed several well-scarred rocks; the evidence of glacier-action is here just as conclusive as in the other parts of the Alps; and the fact of ice moving thus across the country from Mont Blanc to the Jura fortifies the conception previously formed of the magnitude of the phenomenon.

It is, then, perfectly certain that all this mountain region was held by ice, enormous as to mass and in incessant motion. That such an agent was competent to plough out the Alpine valleys cannot, I think, be doubted; while the fact that during the ages which must have elapsed since its disappearance the ordinary denuding action of the atmosphere has been unable, in most cases, to obliterate even the superficial traces of the glaciers, suggests the incompetence of that action to produce the same effect. That the glaciers have been the real excavators, seems to me far more probable than the supposition that they merely filled valleys which had been previously formed by water denudation. Indeed the choice lies between these two suppositions: shall we assume that the glaciers filled valleys which were previously formed by what would undoubtedly be a weaker agent? or shall we conclude that they have been the excavators which have furrowed the uplifted land with the valleys which now intersect it? I do not hesitate to accept the latter view; and this view will carry us still further. According to it the glacier is essentially self-destructive. The more deeply it ploughs the surface of the earth, the more must it retreat. Let the present Alpine valleys be filled to the level of the adjacent ridges, and vast glaciers would again start into existence; but every one of these valleys is a kind of furnace which sends draughts of hot air up to the heights, and thus effectually prevents the formation of ice. While standing on the summit of the Grauhaupt a week or two ago, I was perfectly astonished at the force with which these gusts of heated air rose vertically from the Val du Lys. Marked by the precipitated vapours which chanced to be afloat at the time, the vertical gusts were often as violent as the draught from a factory chimney. Thus, given the uplifted land, and we have a glacial epoch; let the ice work down the earth, every foot it sinks necessitates its own diminution; the glaciers shrink as the valleys deepen; and finally we have a

* That is, in the direction which a local guide pointed out as that of Mont Blanc. The mountain itself was not seen.

state of things in which the ice has dwindled to limits which barely serve as a key to the stupendous operations of a by-gone geologic age. To account for a glacial epoch, then, we need not resort to the hard hypothesis of a change in the amount of solar emission, or of a change in the temperature of space traversed by our system. Elevations of the land, which would naturally accompany the gradual cooling of the earth, are quite competent to account for such an epoch ; and the ice itself, in the absence of any other agency, would be competent to destroy the conditions which gave it birth.

Royal Institution, August 18, 1862.

XXIV. *Mayer, and the Mechanical Theory of Heat.*

By Professor TYNDALL, F.R.S.

MY DEAR JOULE,

ON my return from Switzerland two days ago, I became acquainted with the note which you have published in the last Number of the *Philosophical Magazine*. Would you allow me to make the following remarks in connexion with the subject of it ?

During the spring of the present year I gave at the Royal Institution a course of lectures "On Heat, regarded as a kind of Motion." During the early portion of the course, I had engaged a short-hand writer to report the lectures, with a view to their subsequent publication ; and from this gentleman's notes of my second lecture I make the following extract, which refers to the mechanical theory of heat :—"It is to Mr. Joule, of Manchester, that we are almost wholly indebted for the experimental treatment of this subject. With his mind firmly fixed upon a principle, and undismayed by the coolness with which his first labours appear to have been received, he persisted for years in his attempts to prove the invariability of the relation between heat and ordinary mechanical force. He placed water in a suitable vessel, agitated it by paddles moved by measurable forces, and determined the elevation of temperature ; he did the same with mercury and sperm-oil. He also caused disks of cast iron to rotate against each other, and measured the heat produced by their friction. He urged water through capillary tubes, and measured the heat thus generated. The results of his experiments leave no doubt upon the mind that under all circumstances the absolute amount of heat produced by the expenditure of a definite amount of mechanical force is fixed and invariable." Such has been my language regarding you ; and to it I still adhere. I trust you find nothing in it which indicates a desire on my part to question your claim to the honour

of being the experimental demonstrator of the equivalence of heat and work.

It was not my object in the lecture to which you refer to give a history of the mechanical theory of heat, but simply to place a man of genius, to whom the fates had been singularly unkind, in a position in some measure worthy of him. I was quite aware of all that you have stated regarding Locke, Rumford, Davy, and others: you might have added Bacon to your list. Probably no great generalization was ever established without having first simmered in the minds of many thinkers. But the writings of Mayer form an epoch in the history of this subject; and I certainly should not feel disposed to retract a single sentence that I have written in his favour. I believe he deserves more praise than I have given him. It was he who first used the term "equivalent" in the precise sense in which you have applied it; he calculated the mechanical equivalent of heat from data which, as I have said, "a man of rare ingenuity alone could turn to account;" and his calculation is in striking accordance with your own experimental determinations*. You worked independently of Mayer, and in a totally different way. You brought the mechanical theory to the test of experiment, and in this way proved its truth.

Mayer calculated correctly the mechanical equivalent of heat; but you say that, at the time he wrote, there were no known facts to warrant the hypothesis which he adopted. If by this you mean to say that he made a haphazard guess which had no basis of physical probability, I cannot agree with you. The known constitution of an elastic fluid is, in my opinion, quite sufficient to justify Mayer's proceeding. His hypothesis was this:—Let the quantity of heat required to raise the temperature of gas, preserved *at a constant volume*, t° , be x , and let the heat required to raise the same gas, under *constant pressure*, t° , be $x + y$. The weight raised by the expanding gas in the latter case being P , and the height to which it is raised h , then, according to Mayer,

$$y = P \times h;$$

that is to say, the excess of heat imparted in the latter case is precisely equivalent to the mechanical work performed.

It is undoubtedly implied in this equation that the quantity of heat y is expended wholly in *external* work, and that none of it has been consumed in overcoming internal molecular attractions. This, I think, on the face of it is an extremely probable hypothesis—so probable, indeed, as to amount, in my estimation, almost to a certainty. Clausius makes the same assumption with no better authority than Mayer; and I believe

* The corrected specific heat of air being made use of.

(for I here trust my memory merely) that the assumption has been completely verified by the experiments of the very philosophers who once questioned it. "The law," says Mayer, "'Heat = Mechanical effect,' is independent of the nature of an elastic fluid, which only serves as the apparatus by means of which the one force is converted into the other."

The law of Mariotte was an old principle when Mayer wrote; and the fact of its holding good for gases generally renders the conclusion exceedingly probable that, in yielding to compression, the attractions of the gaseous molecules were insensible; otherwise it is hardly conceivable that the same results could have been obtained with gases so differently constituted: the attractions of the hydrogen atoms, for example, would in all probability be different from those of oxygen. Mayer was further justified in his hypothesis, as to the absence of interior work in the case of a true gas, by the experiments of Ørsted and Despretz, which showed that the law of Mariotte was departed from by the liquefiable gases—the amount of departure depending on the proximity of the gas to its point of condensation. Where, therefore, no departure from the law had been observed (in the case of air for instance), Mayer, I submit, was perfectly warranted in assuming that the molecular attractions were insensible, and that the quantity of heat (y) before referred to was entirely expended in raising the weight, and had its true mechanical equivalent in the weight so raised.

With reference to the application of the mechanical theory of heat to cosmical phenomena, if it were not a liberty, I would ask whether you have ever read the essay of Mayer entitled *Beiträge zur Dynamik des Himmels*? If so, then I have good reason to suspect my competence to come to a correct conclusion as to what constitutes a scientific right.

Knowing that the original memoirs of Mayer would be the true court of appeal in connexion with this subject, I some months ago urged the responsible editor of the Philosophical Magazine to publish translations of them. This I hope he will do; for I quite agree with you in thinking that they would interest many of the readers of the Magazine. Let me add, in conclusion, that I do not think the public estimate of your labours can be in the least affected by any recognition which may be accorded to Mayer. There is room for both of you on this grand platform. Certainly, had Mayer never written a syllable on the mechanical theory of heat, I should not deem your work a whit nobler than I now hold it to be.

Believe me,

Yours, &c.,

JOHN TYNDALL.

Royal Institution,
August 1862.

XXV. *On the Diathermancy of the Media of the Eye.*

By R. FRANZ*.

THE spectrum which is produced by a clear prism exhibits to the eye the greatest intensity of illumination, and hence the maximum of luminous action, in the yellow. Only in a few cases do our thermometric apparatus show the maximum effect of the spectrum in the yellow, but often in the red, and often also outside the red: they even prove that the sun's action in favourable circumstances only ceases at a distance from the red which considerably exceeds the distance of the extreme chemically active rays. How is this phenomenon to be explained on the assumption which so many experiments, and the concurrence of eminent observers in this department has made probable, the theory, namely, that light and heat are identical? Melloni endeavoured to explain the phenomenon of the great intensity of illumination in the yellow by assuming that the retina of the eye has a yellowish colour, which in age becomes pale, and is then replaced by a yellowish coloration of the crystalline lens†. The first part of this assumption would express the conjecture that the retina is more susceptible to the vibrations of the yellow light—in a certain sense, harmonizes more with them than with vibrations of greater or less wave-length.

To this hypothesis of Melloni, Brücke opposed a series of accurate observations. He imagined that the media of the eye do not transmit rays of great wave-length, and therefore that an excitation of the retina by them cannot take place. In order to establish this supposition, he used an Argand burner as a luminous source of heat, while the same lamp covered by a black sheet-iron cylinder furnished the obscure source of heat. A deflection of the needle of the galvanometer, amounting to from 40° to 50° , was observed while the bright source irradiated the thermo-pile directly; but when the cornea was interposed, the deflection was only 8 to 9 degrees; and the deflection was reduced to $1\frac{1}{2}$ degree when the crystalline lens was interposed. When both were simultaneously interposed, no thermal action was produced. When the dark cylinder was placed round the lamp, the galvanometer exhibited no deflection, either when the crystalline lens or the cornea was placed in the path of the thermal rays‡.

Brücke subsequently repeated these experiments in another manner, by placing a dioptric combination of vitreous humour, lens, and cornea in the path of a pencil of solar rays. The deflection of the galvanometer amounted in this case to from 26 to 30

* Translated from Poggendorff's *Annalen*, February 1862.

† Poggendorff's *Annalen*, vol. lvi. p. 574 *et seq.*

‡ Ibid. vol. lxxv. p. 598.

degrees; but when both sides of the combination were blackened, the action on the thermo-pile was zero*.

In 1850, A. Cima published, in the August Number of the Journal *Il Nuovo Cimento*, an investigation of the absorption of the media of the eye for heat, and found that, of 100 rays incident from a Locatelli's lamp, only nine penetrated the three media combined by Brücke. Of the dark rays (the most important in reference to the question proposed above) Cima has taken no account.

Tyndall resumed the investigation in the year 1859. The results obtained agree in general with those published by Brücke. An accurate description of Tyndall's experiments has not, however, been given. Lastly, Janssen published observations on the absorption of heat by the media of the eye. He used a moderator lamp as a source of heat. About 8 per cent. of the incident rays were absorbed by the eye, the media of which were in part investigated separately. In order to determine the thermocrose of the separate parts of the eye, Janssen compared their diathermancy with the diathermancy of water, using different sources of heat. In these experiments a perfect concordance was observed between the absorptive force of a layer of water (between glass plates), and of an equally thick layer of any of the media of which the eye is composed†. But since Melloni has shown that obscure heat can penetrate water‡, it would follow from the last observations that obscure thermal rays can reach the retina. But the heat which is present in the luminous rays could not, according to Masson and Jamin's law § (that perfectly transparent bodies transmit all luminous zones of heat in the same manner), be affected by the absorption which takes place in the interior of the eye, provided that the media of the eye are quite clear. Hence the greater part of the thermal rays absorbed by the eye are obscure.

I have obtained results similar to those of Janssen, but by the application of the solar heat.

A pencil of solar rays was reflected into a dark room, and then decomposed by a prism in order that the zones of different wavelength might be individually tested as to their transmission through the media of the eye. The arrangement of the separate pieces of apparatus was analogous to that described in the *Annalen*, vol. ci. p. 47. The well-known property of rock salt, of possessing the greatest diathermancy for *all* thermal colours, recommends the use of a rock-salt prism in decomposing the

* Poggendorff's *Annalen*, vol. lxi. p. 551.

† *Ann. de Chim. et de Phys.* vol. lx. p. 71.

‡ Poggendorff's *Annalen*, vol. xxiv. p. 645.

§ *Comptes Rendus*, vol. xxxi. p. 14.

sun's rays, when it is a question of working with the largest possible number of those rays. In fact, direct comparison of the spectra of a flint-glass and of a rock-salt prism shows that the heat in the bright zones of the spectra produced by both prisms has equal intensity, but in the dark zones an increase in favour of the rock-salt prism, as I have elsewhere shown*. Nevertheless the absorption of obscure heat by the vesicles of fog, with which, in our latitudes, the atmosphere is perpetually charged, is so great that the differences in the quantity of heat of the individual obscure zones is not so pronounced as might have been expected from Melloni's investigations, which were made under more favourable atmospheric conditions. With a perfectly transparent rock-salt prism, and with an apparently perfectly clear sky, I have never been able to prove an action beyond the sixth dark zone—that is, never at a greater distance from the red limit of the spectrum than the extent of the visible spectrum. But the maximum of the thermal action, using a rock-salt prism, is found in the first dark zone, while, using a flint-glass prism, the maximum is in the red.

At the beginning of these investigations on the absorption of the separate media of the eye, only the results obtained by Brücke were known; and these led me to avoid all glass envelopes, in order to be able to prove the presence of the quantity of obscure heat, in any case small, which penetrates the media of the eye. With this view, the moist parts of the inner eye were poured between two transparent rock-salt plates, and the separate zones of the thermal spectrum produced by a rock-salt prism were investigated. Only during a short series of experiments did the spectrum remain on the screen of the thermo-pile sufficiently clear to be used; for the liquid in the rock-salt trough soon dissolved part of the rock salt, by which irregular refractions were produced. Hence the plates could only be used for one series of experiments. But even the rock-salt prism, although freshly polished before each series of experiments, soon exhibited a less sharp refraction than was necessary for the experiment.

The parts of the eye investigated were in all cases taken from the eye of a recently-killed ox. By an equatorial section the eye was separated from all muscle; the vitreous humour could then be obtained free of any turbidity from the pigment. The upper part of the sclerotic, with the cornea, was then separated from the pigment and stretched. It was, however, soon found that this stretching did not form such a flat surface as to produce on the other side a sharp picture of the spectrum. Hence afterwards, in using the cornea, a small metallic cylinder was used, which, closed at one end by a transparent plate, was

* *Programm of the Berlin Gymnasium zum grauen Kloster, Easter 1858.*

curved at the other in the form of a sphere. A longitudinal slit was made in this spherical curvature, and upon this curvature the cornea was fastened. On the side of the cylinder was a tube, which by means of an India-rubber tube was connected with a bladder full of air. In this manner the cornea was always kept stretched, and the individual zones radiated through it sharply defined.

The aqueous humour had always to be obtained by a prick in the fresh eye; because the humour, as soon as it was poured out from the interior by removing the crystalline lens, always showed a slight turbidity from the injured pigment.

The crystalline, finally, was not used in its natural condition, owing to the difficulty of determining the focus for the dark rays. The experiments with the crystalline were not made with the rock-salt prism, but only afterwards, when the rock-salt prism was replaced by a flint-glass one, when the lens was pressed between two glass plates so that the substance had a thickness of 2 millims.

The first experiments, made in the summer of 1859, excluded all use of glass. Rock-salt prisms and plates were the only bodies which, excepting the media of the eye, were traversed by the thermal rays.

The reflecting galvanometer gave deflections in the red zone which varied between six and eleven divisions of the scale. It is not the object here to find the quantity of heat absorbed by each medium of the eye, but only the ratios in which the various thermal colours traverse the medium in question; hence the number 10 has been chosen for the quantity of the red pencil which has traversed any of the media, and the deflections, as well as the quantities of heat, are reduced to the same unit*.

On account of the smaller dispersive power of the rock-salt prism as compared with the flint-glass prism used in the earlier experiments, in the first experiments on the diathermancy of the cornea every two zones were taken together to exert their heating influence on the thermo-pile. The following relations were obtained for the quantities of heat of the different zones which passed :—

Zone, Violet and Indigo . . .	0·9
„ Blue and Green . . .	3·6
„ Yellow and Red . . .	10·0
First and second dark zone . .	3·7
Third and fourth dark zone . .	0·8

For the *aqueous humour*, which was in a layer of about 4 millims. thickness between two transparent rock-salt plates, the

* Poggendorff's *Annalen*, vol. lxxxix. p. 526.

relation of the quantity of heat in the luminous zones after it had traversed the medium, was very little different from that which a layer of water about 60 millims. thickness had previously given; for the obscure zones, the mean of three series of observations gave,

Red zone	10·0
First dark zone	7·1
Second dark zone	2·6

The *vitreous humour*, lastly, for about the same thickness as the *aqueous humour*, transmitted the heat of the different zones in about the same proportion as the latter.

It appeared probable, therefore, from these experiments, that the non-luminous thermal rays could penetrate the media of the eye, although to a very small extent. But as, from the defective sharpness of the limits of the zones when rock salt was used for the prism and for the sides of the trough, the above experiments had not the certainty of an unfailing result, I decided to replace rock salt by glass. It is clear that, by using glass, a smaller quantity only of the obscure rays could reach the media of the eye; but if in this case a perceptible quantity traverses the media, it follows that the eye cannot be diathermanous for the obscure rays*.

The visible spectrum in the following experiments had an extent of 18 millims. In the screen, which allowed only *one* zone of the spectrum to fall each time on the thermo-pile and retained the other zones, there was a slit of such breadth that the thermo-pile itself was irradiated by a zone 3 millims. in breadth. By a lateral motion of the screen and of the thermo-pile, the different zones could be subjected to investigation.

Placing the *cornea* in the rays from the prism, the following values, for the quantity of heat transmitted, were obtained as the mean from six series of experiments:—

Violet	1·0
Indigo	2·3
Blue	3·7
Green	7·3
Yellow	15·2
Red	10·0
First obscure zone	8·0
Second obscure zone	6·2
Third obscure zone	1·9

* Volpicelli remarks in his *Treatise on Radiant Heat* (*Atti de' nuovi Lincei*, vol. v. p. 145), that he found rock salt less diathermanous for solar heat than flint glass. From comparative experiments which I have made, and which are published in the above-mentioned 'Programme,' this is not

The *aqueous humour* during the investigation was placed in a brass vessel blackened on the inside, and with two parallel glass sides. The relatively better transmission of the yellow rays, which was so decided in the case of the cornea, was not seen in this case. The luminous rays traversed the aqueous humour in proportions equal to those obtained in the case of water. For the dark zone there was obtained as the mean of two observations,

Red zone	10·0
First dark zone	9·1
Second dark zone . . .	4·4
Third dark zone . . .	1·2

The *crystalline lens* was pressed between two glass plates, so that, changing its form, it consisted of a parallel layer 2 millims. thick, enclosed between both glass plates. The lens soon becomes clouded, even if only a few hours have elapsed since the death of the animal. Hence the crystalline lenses from recently-killed animals were subjected to investigation with the utmost rapidity. With the lens, as with the cornea, although not to the same extent, the transmissibility of the heat contained in the yellow zone was comparatively better than had been observed with water. This phenomenon is not, however, to be understood in the sense that the yellow thermal pencil can traverse the crystalline easier than the aqueous humour, but more of the red rays are absorbed, and hence the proportion of the yellow zone increases. If, therefore, for the red zone the number 10 is to be retained as that which expresses the quantity of the heat from this zone which penetrates the lens, the numbers which indicate the quantity of heat which penetrates the other zones must be proportionally larger than we have noticed in the case of water. The mean of three series of experiments was,

Green zone	5·1
Yellow zone	9·9
Red zone	10·0
First obscure zone . . .	8·9
Second obscure zone . .	7·3
Third obscure zone . . .	3·2

In the fourth dark zone, in *one* series, a trace of heat was perceptible.

the case ; but the loss in flint glass cannot be said to be equal to that observed by Melloni for terrestrial sources of heat. Melloni gives for the ratio of the heat radiated through rock salt and flint glass the numbers 100 and 69·64. (Pogg. *Ann.* vol. xxxv. p. 291.)

The last series of experiments referred to the *vitreous humour*. A trough was used like that for the *aqueous humour*. As a mean of four series of observations, the following values were obtained for the ratios of the quantity of heat transmitted:—

Green zone	4·2
Yellow zone	7·6
Red zone	10·0
First obscure zone	9·2
Second obscure zone	6·7
Third obscure zone	2·9
Fourth obscure zone	0·5

From these results it appears that the absorptive powers of the different media of the eye are very similar to that of water; only the cornea and the crystalline appear to absorb more red rays than water does. Hence if we are to hold to the theory of the identity of light and heat, we must look for the reason of the invisibility of those thermal rays of less refrangibility than the red rays in the nature of the retina—revert, in fact, to a supposition like that of Melloni: that the optic nerve cannot give back the slow oscillations of the obscure rays.

I may here be permitted to call attention to a great difficulty, which diminishes the accuracy of results obtained with the obscure rays of the sun. Even if atmospheric air exercises but a small absorption on radiant heat, and air saturated with aqueous vapour has the same diathermancy as dry air*, yet there are in our atmosphere so many fog-vesicles and finely-divided solid substances, that a great quantity of the thermal rays of the sun is lost, either by reflexion or by absorption. Hence, with an apparently clear sky, the distribution of heat in the various spectral zones is very different. I may here adduce two series of observations, the most favourable and unfavourable of several, which were to determine the heat of the zones of the spectra, using a rock-salt prism, and both obtained with an apparently clear sky.

	August 12.	August 21.
Red zone	10·00	10·00
First dark zone	8·94	13·00
Second dark zone	1·11	8·95
Third dark zone	0·26	4·86
Fourth dark zone	1·36
Fifth dark zone	0·73
Sixth dark zone	0·07

* Magnus (Pogg. *Ann.* vol. cxii. p. 540). According to Tyndall's investigations (Pogg. *Ann.* vol. cx. p. 13; Phil. Mag. vol. xxii. p. 276), air saturated with aqueous vapour absorbs a portion of heat. Tyndall (Pogg. *Ann.* vol. cxiv. p. 632; Phil. Mag. vol. xxii. p. 377). Magnus (Pogg. *Ann.* vol. cxiv. p. 635; Phil. Mag. vol. xxiii. p. 250).

The deflections in the red zone amounted to about thirty-five divisions of the scale. It will thus be seen that apparently bright days, like the 12th of August, sometimes cannot be used for experiments with obscure solar heat. On two days only have I obtained such an absorption of the obscure rays, that the absorption in the first obscure zone was greater than in the red. Hence, before each series of experiments like the above, the solar heat which reaches the surface of the earth without any absorbing obstacle had previously to be tested. In this preliminary trial, a phenomenon was observed which rendered a comparison of the results more difficult: the heat in the first obscure zone decreased with the sinking of the sun as compared with the heat in the red, although no difference was perceptible in the external appearance of the sky. In the first obscure zone, on a very bright day, experiment gave the following results for times of observation about an hour apart:—

13·00, 12·26, 11·78, 11·60,

if the quantity of heat contained in the red zone (which was determined anew before each observation) is indicated by 10·00. Similar results were obtained on other days. Hence, on the same day, and under otherwise similar circumstances, the loss of obscure thermal rays accompanying luminous rays increases with the thickness of the layer of our atmosphere which the sun's heat traverses. The general conclusion does not, however, follow, that the quantity of obscure rays always decreases with the thickness of the layer of atmospheric air. For the ratio of the quantities of heat in the first obscure zone and in the red, the mean of all the series of observations in a year was,

In July	10·64 : 10·00
In the first half of August	10·74 : 10·00
In the second half of August . . .	12·87 : 10·00
In September	13·59 : 10·00

The increasing thickness of the layer traversed is not therefore alone the reason of the above observed decrease of the obscure rays; it is to accidental constituents of the atmosphere, fog-vesicles, or finely-divided solid substances that this disturbing influence is due, either by absorption or by reflexion.

We know, from Melloni's investigation and those of other physicists, that the heat which has traversed a layer of a diathermanous substance penetrates a second layer of the same substance the more easily in proportion as the first layer was thicker. Hence if fog-vesicles floating in the air were the cause of the variation in the absorptive power of the atmosphere, less obscure heat ought to be absorbed at midday by a layer of water than at

a later hour of the day, when the sun's rays have traversed a thicker layer of fog-vesicles than at midday. The ratio between the quantities of heat belonging to the first obscure zone which are incident upon the layer of water, and those traversing it, must approach unity from morning to evening.

But experiments by Melloni* and by Volpicelli† contradict this conclusion. Both physicists have found that a layer of water enclosed between two glass plates diminishes the intensity of the transmitted rays from morning to evening. They used the unrefracted heat reflected by a heliostat. In order to extend these experiments to refracted thermal rays I used a rock-salt prism, and also a vessel of rock salt with parallel sides and filled with concentrated solution of rock salt. The inside distance of the sides of the vessel was 6 millims. The rays of the bright zones at midday and at four o'clock traversed this layer of water in the same manner; and even for the rays of the dark zones the differences were within the limits of the errors of observation. The decrease in the intensity of the transmitted rays observed by Melloni and Volpicelli cannot be recognized in these experiments, perhaps partly because the differences in the thickness of the layer of atmospheric air were not so great as in the experiments of the above physicists. However, observations made in the clear atmosphere of Italy are not to be compared, without further consideration, with those made in our latitudes: thus Melloni found the maximum heat in the spectrum of a rock-salt prism so distant from the red end, that the distance between it and the red was as great as the distance between the red and the violet‡, while in my observations the maximum never exceeded the first dark zone.

If, then, the question, what action the particles of water in the atmosphere exercise upon the thermal rays traversing it, remains undecided, it is not to be doubted that small solid particles floating in the atmosphere exercise a great influence upon the rays which traverse the whole atmosphere. A thin cloud of smoke, which scarcely diminished the brilliancy of the solar rays, was traversed by the sun's rays before impinging on the heliostat mirror, upon which *no* heat could be perceived in the dark zones of the spectrum.

From these experiments, it follows that it is not possible to obtain reliable quantitatively comparable results by using as source of heat the sun in our latitudes, and especially in an

* Poggendorff's *Annalen*, vol. lxxxvi. p. 496.

† *Comptes Rendus*, vol. xxxv. p. 953.

‡ Poggendorff's *Annalen*, vol. xxviii. p. 377.

inhabited town. The above experiments do not claim to have obtained a determinate ratio for the quantities of the obscure heat-rays which have passed through the different media of the eye, it is merely shown that a quantity of obscure rays which can be detected by our thermoscopes can reach the retina.

XXVI. *Chemical Notices from Foreign Journals.*

By E. ATKINSON, *Ph.D., F.C.S.*

[Continued from p. 30.]

THE following account* has been given by M. Lamy of his investigations on the new metal discovered by Mr. Crookes.

In examining, three months ago, with Kirchhoff and Bunsen's spectrum-apparatus, a specimen of selenium which my brother-in-law M. Fred. Kuhlmann had extracted from the deposit of the chamber in which sulphuric acid is prepared by the combustion of pyrites, I perceived a well-marked green ray, which I had not perceived in any of the numerous simple or compound mineral bodies which I had studied. I was at that time ignorant that an English chemist, Mr. Crookes, had not only discovered the same green rays under almost analogous circumstances, but had given the name *thallium* to the new element, from the Greek word *θαλλός*, or the Latin *thallus*, frequently employed to express the rich tint of a young and vigorous vegetation. With great sagacity Mr. Crookes had indicated some reactions of the element, which he considered as a metalloid, belonging probably to the sulphur group; but the small quantity of the element on which he had worked had not enabled him to isolate this element and distinguish its true nature.

On my part, I tried to isolate the new body, seeking it in the sediment of the lead chambers, from which had been extracted the selenium that gave the characteristic green line to the spectroscop. This line naturally served as guide in my researches, and permitted me to arrive at the preparation of perfectly definite crystalline compounds, from which, with the aid of the pile, I extracted thallium for the first time.

Properties of Thallium.—Thallium has all the characters of a true metal, and in most of its properties greatly resembles lead. Somewhat less white than silver, its fresh section presents a bright metallic lustre. It appears yellowish when rubbed against a hard body; but this tint is doubtless due to an oxidation, for the metal which has been precipitated by the battery from an aqueous solution and melted in a current of hydrogen is white, with a bluish-grey tint, like that of aluminium.

* *Comptes Rendus*, June 23, 1862.

Thallium is very soft and very malleable; it may be scratched by the nail, and easily cut with a knife. It marks paper; and the mark has a yellow reflexion. Its density, 11.9, is a little higher than that of lead. It melts at 290°, and volatilizes at a red heat. Lastly, thallium has a great tendency to crystallize, for bars obtained by fusion emit a noise like the crackling of tin when they are bent. But the physical property *par excellence* of thallium, that which, according to Bunsen and Kirchhoff's beautiful labours, characterizes the metallic element, that which has led to its discovery, is the faculty it possesses of giving to the colourless gas-flame a green coloration of great richness, and in the spectrum of this flame a single green ray, as isolated and as distinctly marked as the yellow ray of sodium or the red ray of lithium. On the micrometric scale of my spectroscope this ray occupies the division 120.5, that of sodium being at the division 100. The least particle of thallium, or of one of its salts, displays the green line with such lustre that it appears white. From my determinations the $\frac{1}{50000}$ th of a gramme can be perceived in a compound.

Thallium tarnishes rapidly in the air, becoming covered with a thin pellicle of oxide, which protects the rest of the metal from alteration. This oxide is soluble, distinctly alkaline, and has an odour and taste like those of the alkaline metals.

Thallium is attacked by chlorine, slowly at the ordinary temperature, but rapidly at a temperature above 200°. The metal then melts, becomes incandescent under the action of the gas, giving rise to a yellowish liquid, which, on cooling, forms a mass of a paler colour.

Iodine, bromine, sulphur, and phosphorus can also combine with thallium to form iodides, bromides, sulphides, and phosphides.

Recently-prepared thallium retains its metallic lustre in water, which it does not decompose even at the boiling-point; it is decomposed, however, with liberation of hydrogen when an acid is added.

Sulphuric and nitric are the acids which most readily attack thallium, especially when aided by heat. Hydrochloric acid, even when boiling, only dissolves it with difficulty. White soluble salts are formed, the sulphate and nitrate, which crystallize with facility, and a difficultly-soluble chloride, which, however, also crystallizes.

The chloride formed by the direct action of chlorine or by aqua regia is deposited from the aqueous solution in the form of magnificent yellow laminæ, which appear to belong to the rhombohedral system.

Zinc precipitates thallium from the solutions of the sulphate and nitrate in the form of brilliant crystalline laminæ.

Hydrochloric acid and the protochlorides give with the same solutions a white precipitate of chloride of thallium, resembling chloride of silver, but slightly soluble in water; it is, further, little soluble in ammonia, and unaltered by the light.

Hydrosulphuric acid is without action on pure neutral or acid liquids; but if they are alkaline, a voluminous black precipitate of sulphide of thallium is formed, which collects on the bottom of the vessel, and is insoluble in an excess of the precipitant.

Potash, soda, and ammonia do not expel oxide of thallium from its combination with sulphuric or nitric acids.

Natural condition and extraction.—Thallium cannot be considered as very rare in nature. It exists, in fact, in many kinds of pyrites which at present are worked in considerable masses, principally for the manufacture of sulphuric acid. I will mention more especially the Belgian pyrites of Theux, Namur, and Phillippeville. I have also found it in mineralogical specimens from Nantes and Bolivia.

Thallium might be extracted from these pyrites; but it is much more simple to prepare it from the deposits in the lead chambers, where it accumulates in pretty considerable quantities during the manufacture of sulphuric acid.

It is from these thalliferous deposits that I extracted, by a method which I describe in my memoir, the chloride of thallium, which was the starting-point of my investigation.

The metal can be extracted from one of its saline compounds, either by the electric current, or by precipitation with zinc, or by reduction with charcoal at a high temperature. It may also be separated from its chloride by potassium or sodium with the aid of heat; in the latter case the reaction is very brisk.

A small ingot weighing 14 grammes was isolated by a pile of a few of Bunsen's elements—at first from the chlorides which I had originally obtained, then from the crystallized sulphate formed directly by dissolving this thallium in pure sulphuric acid.

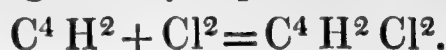
This memoir is not presented to the Academy as a complete research on thallium. My principal object has been to show the new metal, and some of its principal salts. In my next communication I shall try to fill up some of the links in its history.

Berthelot has made* the following additional observations on acetylene†. He finds that it is decomposed by the induction-spark, with separation of carbon, and that mixed with chlorine it

* *Comptes Rendus*, May 12, 1862.

† *Phil. Mag.* vol. xxi. p. 358.

either detonates with deposition of carbon, or unites directly with chlorine, forming an oily liquid like Dutch liquid.



Acetylene is formed by passing chloride of methyle through a tube heated below dull redness, and also by passing carbonic oxide mixed with vapours of hydrochloric acid over siliciuret of magnesium heated to dull redness.

Cupreous acetylene can be formed not only by means of ammoniacal protochloride of copper, but also by protochloride of copper dissolved in chloride of potassium. The formation of the body takes place more easily in the presence of free potash. Cupreous acetylene can be obtained free from chlorine and from nitrogen, but not from oxygen. It is a compound of variable composition, very alterable, and analogous to an oxysulphide. From the analyses made, it appeared to correspond to the formula



The presence of oxygen explains its detonating properties. The detonation takes place at 120° . It gives rise to water, copper, carbon, and carbonic acid mixed with a little oxide of carbon.

Acetylene* exists ready formed in coal-gas, and may be separated from it as cupreous acetylene. Its presence is readily intelligible, inasmuch as coal-gas is formed at a red heat. The proportion is very small, not exceeding a few hundredths of 1 per cent. But its presence is not without influence on the illuminating properties; for, from its composition, $\text{C}^4 \text{H}^2$, which does not differ centesimally from that of benzole, $\text{C}^{12} \text{H}^6$, it is easy to see that its flame is fuliginous, and that a small proportion will communicate considerable luminosity to a gas which is otherwise very feebly luminous. It also contributes very materially to produce the characteristic odour of coal-gas.

According to Pean St. Gilles, when permanganate of potash acts on iodine or hydriodic acid, iodic acid is formed; and he uses this method for the volumetric determination of the latter bodies. He does not, however, explain the reactions involved. Weltzien† states that the reaction with iodide of potassium is expressed as follows:—



The formation of hydrate of potash here is interesting, and the iodate obtained is so pure that this method may be used for its preparation.

* *Comptes Rendus*, May 19, 1862.

† *Ann. der Chem. und Pharm.* vol. cxx. p. 349.

According to Stahlschmidt*, when iodide of nitrogen is mixed with iodide of methyle, the mixture becomes heated if the masses are at all considerable. After some time a brown liquid is formed, from which a pulverulent body separates. On further standing, crystals of the same body are formed. They are of a somewhat dark colour, are difficultly soluble in boiling alcohol, and crystallize out in yellow laminæ.

From the original mother-liquor a second body is gradually deposited in green crystals, which have a remarkable lustre. The author is engaged in the investigation of the composition of these bodies, which may be expected to throw great light on the formula of iodide of nitrogen.

Gelis has described a new mode of preparing hyposulphuric acid†. When carefully washed sulphurous acid is passed into a water containing in suspension hydrated peroxide of iron, a deep-red acid liquor is obtained with a strong odour of sulphurous acid. During the reaction the mixture must be kept cool. For each equivalent of peroxide of iron three equivalents of sulphurous acid are absorbed, and the solution contains the elements of a neutral sulphite of peroxide of iron, $\text{Fe}^2\text{O}^3\text{3SO}^2$, a salt which has not yet been obtained in the dry state. The solution does not retain its red colour; if the flask is corked, it is changed in the course of twenty-four hours into the pale green of salts of the protoxide. The sulphite has become resolved into two salts, the hyposulphate and sulphite of the protoxide of iron. Thus



To obtain hyposulphuric acid or any of its salts, this decolorized liquor may be treated with milk of lime or baryta water, which precipitates iron as the protoxide, and sulphurous and a little sulphuric acid present as sulphite and sulphate. The hyposulphurous acid remains combined with baryta or lime: these salts are readily soluble, and may be easily obtained in the crystalline state. From the baryta- or lime-salt the free acid may readily be prepared by the ordinary methods.

Mendius has investigated a new decomposition of the nitriles‡. These substances take up hydrogen in the nascent state, and are transformed in accordance with the general equation



that is, that the nitrile, which may also be viewed as the cyanide of an alcoholic radical, may be converted into the amine base of

* Poggendorff's *Annalen*, April 1862.

† *Ann. de Chim. et de Phys.*, vol. lxxv. p. 222.

‡ Liebig's *Annalen*, February 1862.

the next higher homologous alcohol. Thus the conversion of cyanide of ethyle into propylamine is expressed as follows:—



The general method was as follows:—A large three-necked Woulfe, provided with a funnel tube and a gas-delivery tube, was about three-fourths filled with granulated zinc; water added, as well as sulphuric acid mixed with the nitrile. The liquid poured off from the mixture was distilled, and the distillate, which contained much undecomposed nitrile, again acted on by acid and zinc. From the solution of sulphate of zinc by far the greater part was separated by crystallization, and nearly all the rest was precipitated by alcohol. The last traces of zinc were separated by means of sulphuretted hydrogen, after adding acetate of soda. The filtrate from the sulphide of zinc was decomposed by caustic soda, and the base which distilled over collected in hydrochloric acid, and thus converted into the hydrochlorate.

By this method, slightly modified in certain cases, Mendius transformed hydrocyanic acid, $\text{C}^1\text{H}^1\text{N}$, into methylamine, $\text{C}^1\text{H}^5\text{N}$; cyanide of methyle or acetonitrile, $\text{C}^2\text{H}^3\text{N}$, into ethylamine, $\text{C}^2\text{H}^7\text{N}$; cyanide of butyle or butyronitrile, $\text{C}^5\text{H}^7\text{N}$, into amylamine, $\text{C}^5\text{H}^{11}\text{N}$. He also effected the transformation of benzonitrile, $\text{C}^7\text{H}^5\text{N}$, into a new base of the composition $\text{C}^7\text{H}^9\text{N}$.

Otto has investigated* some of the derivatives of benzoic and hippuric acids. When hippuric acid is mixed with hydrochloric acid and chlorate of potash added, products are obtained the quantity of chlorine in which depends partly on the quantity of chlorate of potash, and partly on the temperature of the operation. The product of the reaction is an oily liquid consisting of bichlor- and monochlor-hippuric acid, which are easily separated by recrystallization from hot water, the latter being much the more soluble body.

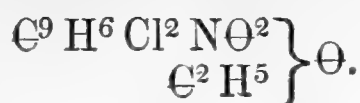
Monochlorhippuric acid, $\text{C}^9\text{H}^8\text{ClN}\Theta^3$, is a viscous yellowish mass, which reddens blue litmus; insoluble in cold, it is soluble in boiling water, and the solution on cooling becomes milky. The salts of the alkalies do not crystallize; the silver, lead, and calcium salts crystallize well.

Bichlorhippuric acid, $\text{C}^9\text{H}^7\text{Cl}^2\text{N}\Theta^3$, can be obtained in a soft granular crystalline mass when allowed to stand for some time under water. The potassium, sodium, calcium, barium, silver, and lead salts all crystallize.

The ether is obtained by dissolving bichlorhippuric acid in absolute alcohol, saturating with hydrochloric acid, and digest-

* Liebig's *Annalen*, May 1862.

ing for some hours in the water-bath. It is a yellowish viscous oil, heavier than water, and with a peculiar odour resembling chlorobenzoic ether. Its formula is



Just as hippuric acid, when boiled with hydrochloric acid, is resolved into benzoic acid and glycol, so chlorhippuric acid is resolved into chlorobenzoic acid and glycol. But the chlorobenzoic acid thus obtained differs from that prepared by the decomposition of chloride of sulphobenzoyl. The body which Otto obtained is transformed by the action of sodium-amalgam into an acid free from chlorine, which is identical in all essential respects with Kolbe and Lautemann's salicylic acid*; and hence Otto regards his chlorobenzoic acid as a peculiar isomeric modification of the true chlorobenzoic acid, a view which is supported by the properties of the nitro-compound prepared from it.

Wurtz† has succeeded in converting aldehyde into alcohol by simply placing a dilute aqueous solution of aldehyde in contact with an amalgam of sodium. The soda formed in this reaction resinified a portion of aldehyde; but another portion was converted into alcohol. The alcohol formed in this reaction was purified by distillation over carbonate of potash, and then over caustic baryta. It had the smell and exactly the boiling-point of ordinary alcohol. Treated by potassium, it disengaged hydrogen and formed ethylate of potassium, from which, by the action of iodide of ethyle, iodide of potassium and ordinary ether were obtained.

Pyroracemic acid, according to its formula $\text{C}^6 \text{H}^4 \text{O}^6$, may be a homologue of glyoxylic acid, $\text{C}^4 \text{H}^2 \text{O}^6$, and Finck‡ has investigated whether its decomposition under the influence of bases is analogous to that of this acid. When a solution of pyrорacemic acid is treated with excess of baryta-water, a yellowish amorphous precipitate is obtained, which is a basic pyrорacemate of baryta, $\text{C}^{18} \text{H}^9 \text{Ba}^3 \text{O}^{18} + \text{BaO HO}$, in the formation of which three molecules of pyrорacemic acid appear to coalesce; and the formula of the modified pyrорacemic acid separated from this precipitate is $\text{C}^{18} \text{H}^{12} \text{O}^{18} = 3(\text{C}^6 \text{H}^4 \text{O}^6)$.

When this basic salt is boiled with excess of baryta in a retort, a decomposition takes place; no volatile products are formed, but instead of the amorphous precipitate a crystalline one of oxalate of baryta appears. The liquid is filtered, the excess

* Phil. Mag. vol. xx. p. 382.

† *Comptes Rendus*, April 28, 1862.

‡ Liebig's *Annalen*, May 1862.

of baryta removed by carbonic acid, and then the baryta combined with acids separated by sulphuric acid. On evaporating the liquid, a yellowish-brown syrup is left, from which a crystalline substance separates. This is recrystallized from aqueous alcohol, dissolved in baryta-water, and again precipitated with hydrochloric acid.

Thus purified it forms a white crystalline powder, very little soluble in hot or cold water, crystallizing from alcohol and ether, in which it is very soluble, in white stellate crystals. It melts at 287° , and sublimes without change, like benzoic acid.

The formula of the acid is $C^{18}H^8O^8$, and Finck names it *Uvitinic acid*; it is isomeric with insolinic acid, but differs in its solubility in ether and alcohol. Finck has investigated several of the salts, of which the silver and copper salts crystallize. It is a bibasic acid, and the formula of the silver salt is $C^{18}H^6Ag^2O^8$.

In the mother-liquor from which uvitinic acid has been separated, another acid, which does not crystallize, is contained. It is a very strong acid, and unites directly with metallic oxides to form neutral salts, which, however, do not crystallize. The formula of this acid appears to be $C^{18}H^{12}O^{14}$. Finck names it *uvitonic acid*.

Comparing the formulæ of the three acids,

Pyroracemic acid	. . .	$C^{18}H^{12}O^{18}$,
Uvitonic acid	. . .	$C^{18}H^{12}O^{14}$,
Uvitinic acid	. . .	$C^{18}H^8O^8$,

it will be seen that the formation of the latter depends on a reduction of the former; for while one part of the pyrорacemic acid is oxidized to oxalic acid, another part loses oxygen and is converted into the other two acids.

According to Simmler*, a highly fluorescent solution may be prepared from cane-sugar as follows. To a syrupy solution of cane-sugar an equal volume of sulphuric acid is gradually added. The mixture blackens, a violent reaction is set up, which results in the formation of a solid mass resembling black peat. When this is somewhat cool, the mass is shaken with about ten times its volume of distilled water and allowed to stand. After filtration a liquor is obtained which in transmitted light is quite colourless, except when strongly concentrated; but viewed in daylight against a black cloth, exhibits a more brilliant blue even than quinine. It appears to be due to a peculiar organic acid, for when neutralized with a base the fluorescence disappears.

* *Chemisches Central-Blatt*, June 18, 1862.

XXVII. *Note on some Objections of Mr. Cayley and Mr. Cockle.*
By G. B. JERRARD*.

HAVING been unwell for some weeks past, I have had no opportunity of expressing what I thought of the objections referred to at the close of my last paper†. In appearance, these objections are most formidable. They were intended to upset a conclusion of mine respecting the existence of an exceptional case, hitherto unobserved by mathematicians, in Lagrange's theory of homogeneous functions. My conclusion, however, still seems to be unshaken. But there is assuredly a notable error somewhere. Let us try to find out with whom it rests.

1. The objection which comes first in order is so peculiar, that I am quite unable to characterize it beforehand, or to reduce it to any known class.

"Putting, for shortness," says Mr. Cayley,

$$\alpha = f(\iota) f(\iota^2) f(\iota^3) f(\iota^4),$$

I understand Mr. Jerrard to mean that α , α^5 being given as the roots of two equations of the sixth order respectively, α cannot be expressed as a rational function of α^5 and of the coefficients of the two equations; or, what is the same thing, since the coefficients of each equation are rational functions of the coefficients of the given equation of the fifth order, that α cannot be expressed as a rational function of α^5 and of the coefficients of the equation of the fifth order. If this is not Mr. Jerrard's meaning, he will doubtless set me right; but that α can be so expressed seems so clear, that I must apologize for giving a formal proof of it‡.

The question, however, relating to α is, as we shall see a little further on, very different from what it is here implicitly assumed to be.

2. Let us suppose, in conformity with my previous notation for $n=6$, $m=5$ §, that u and v designate two rational six-valued homogeneous functions of the roots of the equation

$$x^5 + A_1x^4 + A_2x^3 + \dots + A_5 = 0;$$

then by Lagrange's theory we can generally express either of these functions in rational terms of the other and of the coefficients A_1, A_2, \dots, A_5 . The only exception, according to that theory, is when there are equal roots in (U) or (V), the equations on which u, v respectively depend,—or rather when the number of unequal roots in (U) differs from that of unequal roots in (V).

3. But I maintain that when u and v are taken equal to those

* Communicated by the Author.

† Philosophical Magazine for June 1862.

‡ Ibid. for March 1862.

§ Ibid. for May 1861.

particular six-valued functions represented by

$$\alpha, \alpha^5,$$

the theory of Lagrange will afford us no aid whatever in establishing a rational communication between them, although in this case the equations (U), (V) have undoubtedly no equal roots. Vain, I assert, must every attempt be to proceed beyond

$$\alpha^5 = (\alpha)^5.$$

4. That there is something exceptional in the case in question, we may see at once without entering into the calculations.

For beside the equation

$$v = \mu_5 + \mu_4 u + \mu_3 u^2 + \dots + \mu_0 u^5 \dots \dots \dots (e_1)$$

for expressing v in rational terms of u ,—which equation in ordinary cases would, if there were no equal roots in (U), (V), be sufficient for the purpose intended,—we have, in the present instance,

$$v = u^5 \dots \dots \dots (E_1)$$

Does (E₁), I would ask, exert no disturbing influence on the coefficients of (e₁)?

Again, in ordinary cases we can generally obtain a single definite equation of the form

$$u = \nu_5 + \nu_4 v + \nu_3 v^2 + \dots + \nu_0 v^5; \dots \dots \dots (e_2)$$

but here we set out with supposing that

$$u = \sqrt[5]{v} : \dots \dots \dots (E_2)$$

that is to say, u is to be both a rational and an irrational function of v .

What wonder, therefore, if, in a case so anomalous, Lagrange's theory should furnish not only illusory but even fallacious results. And, indeed, on a closer inspection of the subject, surmise will quickly give place to certainty.

5. Combining (e₁), (E₁), we have

$$0 = \mu_5 + \mu_4 u + \mu_3 u^2 + \dots + (\mu_0 - 1)u^5; \dots \dots (e'_1)$$

in which $\mu_5, \mu_4, \dots \mu_0$ must all of them, in accordance with what has been stated in art. 2, be expressible as rational functions of $A_1, A_2, \dots A_5$.

But as (U), the equation on which u depends, is rationally irreducible below the sixth degree, the coefficients

$$\mu_5, \mu_4, \dots (\mu_0 - 1)$$

of the equation (e'₁) ought severally to vanish.

When, therefore, u, v are equal to α, α^5 respectively, (e₁) ought to become

$$v = u^5,$$

which is precisely identical in form with the equation (E₁) itself.

We thus perceive that we cannot derive any aid from Lagrange's theory in expressing α^5 as a rational function of α and of $A_1, A_2, \dots A_5$. It remains to show that (e_2) also is inapplicable when $u=\alpha, v=\alpha^5$.

The equation (e_2) is supposed to be obtained directly from the theory of Lagrange. But an equation identical in its ultimate form with (e_2) ought to be found by combining (e_1) and (U). Whence

$$u = \frac{0}{0}; \dots (e_1, U)'$$

if

$$u=\alpha, \quad v=\alpha^5,$$

since (e_1) here becomes, or at least ought to become, $\alpha^5=\alpha^5$.

Of the interpretation of $(e_1, U)'$, which is indicative of an incongruity, I intend to speak in another place.

6. The curious irrelevancy of Mr. Cayley's objection will now be seen. He merely occupies himself in proving what I had taken for granted, while he leaves untouched the main question to be discussed, which is this:—*Is Lagrange's theory inapplicable to the case of $u=\alpha, v=\alpha^5$, in virtue of the equation (e'_1) ?*

7. Mr. Cockle, indeed, notices this equation in a separate paper in the same Number of the Philosophical Magazine as that which contains Mr. Cayley's objection. But the meaning of (e'_1) is so misapprehended by Mr. Cockle, that the very existence of the equation might just as well have been ignored at once.

His words are:—"Mr. Jerrard's paper in this Journal for May 1861 does not disprove a rational communication between the resolvent product and its fifth power. His equation (e'_1) is not illusory but identical, and we may expunge it and substitute u^5 for v in the second of the equations marked (e). The result of this substitution, reduced by means of (U), may be made to take the form

$$\lambda_5 + \lambda_4 u + \lambda_3 u^2 + \dots + \lambda_0 u^5 = 0;$$

and the six linear, but not homogeneous, equations

$$\lambda_5=0, \quad \lambda_4=0, \dots \lambda_0=0$$

will determine the six quantities $\nu_5, \nu_4, \dots \nu_0$."

Now why is (e'_1) not illusory but identical? In the paper of mine here referred to, I arrived at $(e_1, U)'$ by a slightly different route from that pursued in art. 5. "Combining," I said, "the equations (e_1) , (U), that is to say, (e'_1) , (U), we find

$$u = \frac{0}{0};$$

the equation (e'_1) being, as we see, illusory." Accordingly he finds it necessary for his argument that (e'_1) should be not illu-

sory but identical. It is true that the equation (e_1) , not (e'_1) , is, or ought to be, identical in the case under discussion. But what of that? I can discover no semblance of solidity in his remark. Doubtless nothing better can be advanced on his side of the question. For (e'_1) cannot be expunged.

With respect to what he says of the six equations and the six quantities to be determined, I would merely remark that, when v can be expressed as a rational function of u , we ought, in general, to be able to obtain an analogous expression for u in terms of v . The method is one of verification in ordinary cases.

8. It is clear, from what has been said, that if v , instead of being equal to u^5 , be a rational function of u defined by

$$v = a_5 + a_4 u + a_3 u^2 + \dots + a_0 u^5,$$

Lagrange's theory will be inapplicable in virtue of the equation

$$0 = \mu_5 - a_5 + (\mu_4 - a_4)u + (\mu_3 - a_3)u^2 + \dots + (\mu_0 - a_0)u^5.$$

We cannot superadd a second rational relation to the first.

What mathematician, who is not urged on by his fear of the algebraical resolution of equations, can doubt this?

August 1862.

XXVIII. *On the Principle of Discontinuity in Solutions of Problems in the Calculus of Variations.* By Professor CHALLIS, F.R.S.*

THE following communication was written in consequence of my receiving, from Professor Lindelöf of Helsingfors, a copy of the work, recently published by him in conjunction with M. l'Abbé Moigno, entitled *Leçons de Calcul des Variations*. I believe that I owe this mark of courtesy (for which I beg to take the present opportunity of expressing my acknowledgement) to the circumstance of my having given, in the Number of the Philosophical Magazine for August 1861, a solution of the following problem:—To determine the surface of revolution of given superficial area, and passing through two given points of the axis, which shall include the greatest volume. The solution there proposed is adverted to in p. 224 of the work in these terms:—“On their part, MM. Challis and Todhunter, admitting that the line which is to unite the points A and B is composed, first, of two straight lines raised perpendicularly to the axis at the points A and B, then of a curve joining the extremities of these straight lines, equally arrive at giving a discontinuous curve as the solution of the problem in question.” Respecting this statement, I beg permission to make the remark that it takes no notice of an important difference between Mr. Todhunter's treatment of the

* Communicated by the Author.

problem and that which my solution exhibits. Mr. Todhunter *assumes* a certain discontinuity of the joining line, after which it becomes a very simple matter to obtain a solution; whereas in the process which I have employed that particular discontinuity is *inferred*. I make this remark expressly for the purpose of raising the question as to whether the solutions of problems in the Calculus of Variations may involve the principle of discontinuity. This being a question of a novel and interesting character, I propose now to discuss it at some length, and to give my reasons for answering it affirmatively.

Before entering upon considerations more directly bearing upon the subject for discussion, I beg to advert to an assertion, noticed by M. Lindelöf, and which I cannot but regard as unscientific, to the effect that the Calculus of Variations *fails* to give a solution of the problem above enunciated. Upon the general principles of analytical calculation it may, on the contrary, be asserted that, if there be *in rerum naturâ* a quantitative maximum or minimum, there must be appropriate rules by which its amount may be calculated either exactly or approximately. We may fail in our knowledge of the rules; but calculation cannot be said to fail. This general remark may be illustrated by the following example.

It is known, from the practice of navigation, that the brachystochronous course of a sailing-vessel from one point to another is generally a broken line. How, it may be inquired, does analytical calculation determine such a course? Let A' and B' be the two points, A' being the point of starting; and let this point be also the origin of coordinates. Then if the axis of x be in the direction in which the wind blows, and p be the tangent of the angle which the ship's course at any instant makes with that axis, the rate of sailing is some function of this angle, and therefore of p . As, however, the rate is the same, whether the direction of sailing makes a certain angle, or its supplement, with the axis of x , the wind in the two cases striking on opposite sides of the ship, we shall have

$$\text{rate of sailing} = f(p^2),$$

which equation expresses all the conditions of the problem. Hence the quantity to be a minimum is

$$\int \frac{\sqrt{1+p^2}}{f(p^2)} dx.$$

The differential equation given by the rules of the Calculus of Variations for this instance is

$$\frac{p}{\sqrt{1+p^2} f(p^2)} - \frac{2p \sqrt{1+p^2} f'(p^2)}{\{f(p^2)\}^2} = C.$$

This equation, containing only p and constants, shows that the brachystochronous course is *rectilinear*; and with this result nautical practice agrees. Also it is to be remarked that the case in which the wind blows directly from A' to B' , in which case the straight line joining these points is plainly a minimum or maximum course whatever be the function $f(p^2)$, must be included in the general investigation. Hence it follows that the equation is satisfied if $p=0$, and consequently that $C=0$. Thus after getting rid of the factor p , we shall have

$$f(p^2) - 2(1+p^2)f'(p^2) = 0.$$

As this result is of the form $\psi(p^2)=0$, and is therefore equally satisfied by positive and negative values of p , it is the differential equation of an even number of straight lines which, taken in pairs, make supplementary angles with the axis of x . The values of the angles are deducible from the equation above, if the form of $f(p^2)$ be explicitly given. It thus appears that analysis indicates, respecting the brachystochronous course, that it consists of not fewer than two straight lines. But we have also to satisfy the condition that the course passes through the given points A' and B' . Now this may be done by means of two such lines inclined at supplementary angles to the axis of x , the ship's course being along one up to the point of their intersection, and subsequently along the other. This result is also in accordance with practical sailing. If it be objected that the course, being on different lines, is discontinuous, we may reply as follows. The Calculus of Variations answers the question as to the nature of the course, only so far as to give a differential equation between the variables, which equation requires to be afterwards solved by appropriate rules. Now it is known, from the theory of such equations, that the solutions in certain cases represent more than one line. Hence, if by means of more than one line the conditions of a proposed problem may be satisfied, and an answer be obtained which possesses the character of a maximum or minimum, there would seem to be no analytical reason why such solution should not be accepted. In the instance before us, it may be thus shown that the solution given by two straight lines, determined as to direction by the foregoing equation, possesses the character of a minimum. Let x and a be respectively the coordinates parallel to the axis of x of the point of intersection of the lines and the point B' . Then the time of sailing from A' to B' is

$$\frac{x \sqrt{1+p^2}}{f(p^2)} + \frac{(a-x) \sqrt{1+p^2}}{f(p^2)}, \text{ or } \frac{a \sqrt{1+p^2}}{f(p^2)}.$$

If this be a minimum, we shall have

$$\frac{ap \frac{dp}{dx}}{(f(p^2))^2 \sqrt{1+p^2}} (f(p^2) - 2(1+p^2) f'(p^2)) = 0.$$

The equation obtained by equating to zero the factor in brackets, which alone gives a value of p depending on the function $f(p^2)$, is identical with that deduced by the Calculus of Variations. This instance may suffice to show that the processes of the Calculus of Variations embrace solutions in which the principle of discontinuity is involved. Although, in the solution of the problem mentioned at the beginning of these remarks, the discontinuity enters in a different manner from that exhibited above, the foregoing reasoning may, I think, be accepted as evidence that no antecedent objection lies against a solution on the ground that it involves discontinuity. I proceed now to consider further the question of discontinuity with reference to the other problem.

For this instance, the Calculus of Variations gives, as is known, the differential equation

$$2ay - (b - y^2) \sqrt{1+p^2} = 0,$$

a and b being arbitrary constants. This equation, if it could be exactly integrated, would give a relation between x and y involving three constants, all of which would be necessary for completely answering the proposed question. Although such an integral is not obtainable, the following exact equation is deducible by integration, as I have shown in the communication before quoted, from the above differential equation, viz.

$$\cos \left(\frac{s}{a} + k \right) = \frac{2a^2 + b - y^2}{2a \sqrt{a^2 + b}},$$

s being the arc of the curve, and k an arbitrary constant introduced by the integration. Now it is here to be remarked that this equation proves at once that the curve does not pass through the points A and B, because in that case we should have $y=0$, and the right-hand side of the equation would be numerically greater than unity; which is impossible. The ordinates y_0, y_1 at those points must, therefore, have certain unknown values which it is required to find. To accomplish this, it must, first, be admitted that the total enclosing surface of the solid is composed of the surface generated by the revolution of the curve, and the surfaces generated by the revolution of the extreme ordinates through A and B. Against this admission no argument can be adduced except an *à priori* objection, for which no reason has hitherto been assigned, against the principle of discontinuity.

Such objection is, I think, completely removed by the reasoning applied to the former problem. Consequently, the discontinuity being for these reasons admitted, it will be necessary to obtain a new expression for the differential of the surface. That is, under the sign of integration there will be the additional terms $ar dr + ar' dr'$, r and r' being the distances of points of the circular areas from A and B respectively, and the integrations being taken from $r=0$ to $r=y_0$, and from $r'=0$ to $r'=y_1$. The quantity outside the sign of integration to be equated to zero will thus be

$$\left(\frac{p}{\sqrt{1+p_1^2}} + 1\right) ay_1 \delta y_1 - \left(\frac{p_0}{\sqrt{1+p_0^2}} - 1\right) ay_0 \delta y_0;$$

and as y_0 and y_1 are not given, we shall have

$$\frac{p_1}{\sqrt{1+p_1^2}} + 1 = 0, \text{ and } \frac{p_0}{\sqrt{1+p_0^2}} - 1 = 0.$$

Hence it follows that $p_0 = +\infty$ and $p_1 = -\infty$, and that the extreme ordinates are tangents to the curve. The original differential equation shows, by putting $p = \infty$, that the values of these ordinates are determined by the equation $y^2 = b$. It has been found by M. Delaunay, as stated by M. Lindelöf in p. 220, that the curve might be described by one of the foci of an hyperbola rolling upon the axis of x . A line so described would be a closed curve; but according to the foregoing reasoning, only the portion concave to the axis is applicable to the proposed question.

The problem has thus received a unique and definite answer by a process each step of which is necessarily true if the principle of discontinuity be admissible. The reasoning has also shown that the assumption usually made in the treatment of this problem, that $b=0$, is not allowable, and that it simply has the effect of making an answer to the question impossible. Also I am unable, for the following reasons, to assent to the considerations in pp. 224 and 225, by which M. Lindelöf endeavours to show that the problem does not admit of a solution. It is evident that a surface generated by the revolution about the axis of x of a line connecting A and B might be of given superficial extent, and at the same time enclose as small a solid as we please; for instance, if the line were a portion of an ellipse of great eccentricity, with its major axis parallel to, and extending beyond, the straight line joining A and B. And it is equally evident that the surface cannot enclose as large a solid as we please. There must, therefore, be a particular surface which encloses the limiting maximum. Not perceiving any reason for conceding that the discovery of this maximum is out of the reach

of the Calculus of Variations, I maintain that that particular surface is the composite one to which the foregoing reasoning has conducted, and that the principle of discontinuity which the reasoning involves must be recognized. An inference of this importance invests the discussion of the problem with peculiar interest.

Cambridge, August 6, 1862.

XXIX. On the Application of the Theorem of the Equivalence of Transformations to the Internal Work of a mass of Matter. By Professor R. CLAUSIUS.

[[Concluded from p. 97.]

§ 7. **WE** will now investigate the manner in which, from equation (II.), it is possible to arrive at the equation (I.) previously given in § 1, which equation must apply, according to the fundamental theorem that I have already enunciated, to every *reversible circular process*.

When successive alterations of condition constitute a *circular process*, the disgregation of the body is the same at the end of the operation as it was at the beginning, and hence the following equation must hold good:—

$$\int dZ = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (15)$$

Equation (II.) is hereby transformed into

$$\int \frac{dQ + dH}{T} = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

In order that this equation may accord with equation (I.), namely,

$$\int \frac{dQ}{T} = 0,$$

the following equation must be applicable to every reversible circular process:—

$$\int \frac{dH}{T} = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (III.)$$

It is this equation which leads to the consequences referred to in the introduction as at variance with commonly received views. It can, in fact, be proved that, in order that this equation may be true, it is at once *necessary* and *sufficient* to assume the following theorem:—

The quantity of heat actually present in a body depends only on its temperature, and not on the arrangement of its component particles.

It is at once evident that the assumption of this theorem suff-
Phil. Mag. S. 4. Vol. 24. No. 160. Sept. 1862. P

fices for equation (III.) ; for if H is a function of the temperature only, the differential expression $\frac{dH}{T}$ takes the form $f(T)dT$, in which $f(T)$ is obviously a real function which can have but one meaning, and the integral of this expression must plainly be equal to nothing if the initial and final values of T are the same.

The necessity of this theorem may be demonstrated thus.

In order to be able to refer the alterations of condition to alterations of certain magnitudes, we will assume that the manner in which the body changes its condition is not altogether arbitrary, but is subject to such conditions that the condition of the body is determined by its temperature, and by any second magnitude which is independent of the temperature. This second magnitude must plainly be connected with the arrangement of the constituent particles: we may, for example, consider the disgregation of the body as such a magnitude; it may, however, be any other magnitude dependent on the arrangement of the constituent particles. A case which often occurs, and one which has been frequently discussed, is that in which the *volume* of the body is the second magnitude, which can be altered independently of the temperature, and which, together with the temperature, determines the condition of the body. We will take X as a general expression for the second magnitude, so that the two magnitudes T and X together determine the condition of the body.

Since, however, the quantity of heat, H , present in the body is a magnitude which in any case is completely determined by the condition of the body at any instant, it must here, where the condition of the body is determined by the magnitudes T and X , be a function of these two magnitudes. Accordingly, we may write the differential dH in the following form,

$$dH = MdT + NdX, \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

where M and N are functions of T and X , which must satisfy the well-known equation of condition to which the differential coefficients of a function of two independent variables are subject; that is, the equation

$$\frac{dM}{dX} = \frac{dN}{dT}. \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

Again, if the integral $\int \frac{dH}{T}$ is to become equal to nothing each time that the magnitudes T and X return to the same values as they had at the beginning, $\frac{dH}{T}$ must also be the complete differential of a function of T and X . And since we may write, as

a consequence of (17),

$$\frac{dH}{T} = \frac{M}{T} dT + \frac{N}{T} dX, \quad . \quad . \quad . \quad . \quad . \quad (19)$$

we obtain, for the differential coefficients which here occur, the equation of condition

$$\frac{d}{dX} \left(\frac{M}{T} \right) = \frac{d}{dT} \left(\frac{N}{T} \right), \quad . \quad . \quad . \quad . \quad . \quad (20)$$

which exactly corresponds to equation (18).

By carrying out the differentiations, this equation becomes

$$\frac{1}{T} \frac{dM}{dX} = \frac{1}{T} \frac{dN}{dT} - \frac{N}{T^2}; \quad . \quad . \quad . \quad . \quad . \quad (21)$$

and, by applying equation (18) to this, we get

$$N = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (22)$$

According to (17), N is the differential coefficient of H according to X ; and if this differential coefficient is to be generally equal to nothing, H itself must be independent of X ; and since we may understand by X any magnitude whatever which is independent of T , and together with T determines the condition of the body, it follows that H can only be a function of T .

§ 8. This last conclusion appears, according to commonly received opinions, to be opposed to well-known facts.

I will choose as an illustrative example, in the first place, a case which is very familiar, and in which the discrepancy is particularly great, namely, water in its various states. We may have water in the liquid state, and in the solid state in the form of ice, at the same temperature; and the above theorem asserts that the quantity of heat contained in it is in both cases the same. This appears to be contradicted by experience. The specific heat of ice is only about half as great as that of liquid water, and this appears to furnish grounds for the following conclusion. If at any given temperature a unit of weight of ice and a unit of weight of water in reality contained the same quantity of heat, we must, in order to heat or cool them both, impart to or withdraw from the water more heat than we impart to or withdraw from the ice, so that the equality in the quantity of heat could not be maintained at any other temperatures. A similar difference to that existing between water and ice also exists between water and steam, inasmuch as the specific heat of steam is much smaller than that of water.

To explain this difference, I must recall the fact that only part of the heat which a body takes up when heated goes to increase the quantity of heat actually present in it, the remainder being consumed as work. I believe now that the differences in

the specific heat of water in its three states of aggregation is caused by great differences in the proportion which is consumed as work, and that this proportion is considerably greater in the liquid state than in the other two states*. We must, accordingly, here distinguish between the observed specific heat and the true specific heat with which the alteration of temperature dT must be multiplied, in order that we may obtain the corresponding increase of the quantity of heat actually present; and, in accordance with the above theorem, I believe we must admit that the true specific heat of water is the same in all three states of aggregation; and the same considerations as apply to water must naturally also apply in like manner to other substances. In order to determine experimentally the true specific heat of a substance, it must be taken in the form of strongly overheated vapour, in such a state of expansion, in fact, that the vapour may, without sensible error, be regarded as a perfect gas; and its specific heat must then be determined under constant volume.

Rankine is not of my opinion in relation to the specific heat of bodies in different states of aggregation. At page 307 of his 'Manual of the Steam-Engine,' he says, "The real specific heat of each substance is constant at all densities, so long as the substance retains the same condition, solid, liquid, or gaseous; but a change of real specific heat, sometimes considerable, often accompanies the change between any two of these conditions." In the case of water in particular, he says, on the same page, that the true specific heat of liquid water is "sensibly equal" to the apparent specific heat; whereas, according to the view above put forth by myself, it must amount to less than half the apparent specific heat.

If Rankine admits that the true specific heat may be different in different states of aggregation, I do not see what reason there

* I have already enunciated this view in my first memoir on the Mechanical Theory of Heat, having, in fact, inserted the following in a note (Poggendorff's *Annalen*, vol. lxxix. p. 376), which has reference to the diminution of the cohesion of water with increase of temperature:—"Thence it follows, also, that only part of the quantity of heat which water receives from without when heated, is to be regarded as heat in the free state, while the rest is consumed in diminishing the cohesion. In accordance with this view is the circumstance that water has so much higher a specific heat than ice, and probably also than steam." At that time the experiments of Regnault on the specific heat of the gases were not yet published, and we still found in the text-books the number 0.847, obtained by De la Roche and Bérard, for the specific heat of steam. I had, however, already concluded, from the theoretical grounds which are the subject of the present discussion, that this number must be much too high; and it is to this conclusion that the concluding words, "and probably also than steam," refer.

is for supposing it to remain constant within the same state of aggregation. Within one and the same state of aggregation, *e. g.* within the solid state, alterations in the arrangement of the molecules occur, which, though without doubt less considerable, are still essentially of the same kind as the alterations which accompany the passage from one state of aggregation to another; and it therefore seems to me that there is something arbitrary in denying for the smaller alterations what is admitted in respect to the greater. On this point I cannot agree with the way in which the talented English mathematician treats the subject; but, relying simply on the theorem established by myself in relation to the working force of heat, it appears to me that only one of the following cases can be possible. Either this theorem is correct, in which case the true specific heat remains the same, not only for the same state of aggregation, but for the different states of aggregation, or the theorem is not correct, and in this case we have no definite knowledge whatever concerning the true specific heat, and it may equally well be variable within the same state of aggregation as in different states of aggregation.

§ 9. I believe, indeed, that we must extend the application of this theorem, supposing it to be correct, still further, and especially to *chemical combinations and decompositions*.

The separation of chemically combined substances is likewise an increase of the disgregation, and the chemical combination of previously isolated substances is a diminution of their disgregation; and consequently these processes may be brought under considerations of the same class as the formation or precipitation of vapour. That in this case also the effect of heat is to increase the disgregation, results from many well-known phenomena, many compounds being decomposable by heat into their constituents—as, for example, mercuric oxide, and, at a very high temperature, even water. To this it might perhaps be objected that, in other cases, the effect of increased temperature is to favour the union of two substances—that, for instance, hydrogen and oxygen do not combine at low temperatures, but do so easily at higher temperatures. I believe, however, that the heat exerts here only a secondary influence, contributing to bring the atoms into such relative positions that their inherent forces, by virtue of which they strive to unite, are able to come into operation. Heat itself can never, in my opinion, tend to produce combination, but only, and in every case, decomposition.

Another circumstance which renders the consideration of this case more difficult is this, that the conclusions we have been accustomed to draw always imply that the alterations in question can take place in a constant and reversible manner; this,

however, is not usually the case under the circumstances which accompany our chemical operations. Nevertheless cases do occur in which this condition is fulfilled, especially in the chemical changes brought about by the action of electric force. The galvanic current affords us a simple means of causing combination or decomposition; and in this case the cell in which the chemical change takes place itself forms a galvanic element, the electromotive force of which either contributes to intensify the current, or has to be overcome by other electromotive force; so that in the one case there is a production, and in the other a consumption of work.

Similarly, I believe that we could in all cases, by producing or expending work, cause the combination or separation of substances at pleasure, provided we possessed the means of acting at will on the individual atoms, and of bringing them into whatever position we pleased. At the same time I am of opinion that heat, leaving out of view its secondary effects, tends in a definite manner, in all cases of chemical change, to render the combination of atoms more difficult, and to facilitate their separation, and that the energy of its action is likewise regulated by the general law above given.

Supposing this to be the case, the theorem which we have deduced from this law must also be applicable here, and a chemical compound must contain exactly the same quantity of heat as its constituents would contain at the same temperature in the uncombined state. Hence it follows that the true specific heat of every compound must admit of being simply calculated from the specific heats of the simple bodies. If we further take into consideration the well-known relation between the specific heats of the simple bodies and their atomic weights (a relation which I believe not only to be nearly, but, in the case of the true specific heats, absolutely exact), it is apparent what enormous simplifications the law which we have established is capable, supposing it to be true, of introducing into the doctrine of heat.

§ 10. After these expository remarks, I can now cite the more extended form of the theorem of the equivalence of transformations.

In § 1 I have mentioned two kinds of transformations: first, the transformation of work into heat, and *vice versa*; and secondly, the transference of heat between bodies of different temperatures. In addition to these, we will now take, as a third kind of transformation, the alteration of the disgregation of a body, assuming the increase of disgregation as a positive, and the diminution of it as a negative, transformation.

We will now, in the first place, bring the first and last transformation into relation with each other; and here the same

circumstances have to be taken into consideration as have already been discussed in § 5. If a body changes its disgregation in a reversible manner, the change is accompanied by a transformation of heat into work, or of work into heat, and we can determine the equivalent values of the two kinds of transformations by comparing together the transformations which take place simultaneously.

Let us first assume that *a constant alteration of arrangement takes place at different temperatures*; the quantity of heat which is thereby converted into work, or is produced from work, is then different in the different cases, and is, in fact, according to the above law, proportional to the absolute temperature. If, now, we regard as equivalent the transformations which correspond to one and the same alteration of arrangement, it results that, for the determination of the equivalent values of these transformations, we must divide the several quantities by the absolute temperatures respectively corresponding to them. The production of the quantity of heat Q from work must, therefore, if it takes place at the temperature T , have the equivalent value

$$\frac{Q}{T} \text{ const.};$$

and if we here take the constant, which can be assumed at will, as equal to unity, we obtain the expression given in § 1.

We will assume, in the second place, that *various alterations of arrangement occur at a constant temperature*, these alterations being accompanied by increase of disgregation; and if we adopt as a principle that increments of disgregation wherein the same quantity of heat is converted into work shall be regarded as equivalent to each other, and that their equivalent value shall be equal, when taken absolutely, to that of the simultaneously occurring transformation from heat into work, but that they shall have the opposite sign, we thus acquire a starting-point for the determination of the equivalent values of alterations of disgregation.

By combining these two rules, we can determine also the equivalent value of an alteration of disgregation occurring at various temperatures, and we thus obtain the expression given in § 5. Let, for instance, dL be an element of the work performed during an alteration of disgregation, in effecting which the quantity of heat $A dL$ is consumed, and let the equivalent value of the alteration of disgregation be denoted by $Z - Z_0$, we then have

$$Z - Z_0 = A \int \frac{dL}{T}.$$

Finally, as to the process cited above as the second kind of transformation—namely, the passage of heat between bodies of

different temperatures,—in the case of reversible alterations of condition it can be brought about only by heat being converted into work at the one temperature, and work back again into heat at the other; it is therefore already comprised among the transformations of the first kind. And, as I have mentioned in my previous memoir, we may in all cases regard a transformation of the second kind as a combination of two transformations of the first kind.

We will now return to equation (II.), namely,

$$\int \frac{dQ + dH}{T} + \int dZ = 0.$$

dH is here the increment received by the quantity of heat present in the body during an infinitely small change of condition, and dQ is the quantity of heat simultaneously given up to external bodies. The sum $dQ + dH$ is therefore the quantity of heat which, supposing it to be positive, is freshly produced from work, or if it is negative, must be converted into work. Accordingly, the first integral in the above equation is the equivalent value of all the transformations which have occurred of the first kind; the second integral represents the transformations of the third kind; and the sum of all these transformations must be, as is expressed by the equation, equal to nothing.

Hence, in so far as it regards *reversible* alterations of condition, the theorem may be expressed in the following form:—

If the equivalent value $\frac{Q}{T}$ be assumed for the production of the quantity of heat Q from work at the temperature T , a magnitude admits of being introduced, as the second transformation corresponding thereto, which has relation to the alterations of the condition of the body, and is completely determined by the initial and final conditions of the body, and which fulfils the condition that in every reversible alteration of condition the algebraic sum of the transformations is equal to nothing.

§ 11. We must now examine the manner in which the foregoing theorem is modified when we give up the condition that all alterations of condition are to take place reversibly.

From what has been said in § 4 concerning non-reversible alterations of condition, it is easy to perceive that the following must be the general behaviour of all three kinds of transformations. A negative transformation can never occur without a simultaneous positive transformation whose equivalent value is at least as great; on the other hand, positive transformations are not necessarily accompanied by negative transformations of equal value, but may take place in conjunction with smaller negative transformations, or even without any at all.

If heat is to be transformed into work, which is a negative transformation, a positive alteration of disgregation must take place at the same time, which cannot be smaller in amount than that determinate magnitude which we regard as equivalent. In the positive transformation of work into heat, on the other hand, the state of things is different. If the force of heat is overcome by opposed forces, so that a negative change of disgregation is brought about, we know that in this case the overcoming forces may be greater than is required to produce the particular result. The excess of force may then give rise to motions of considerable velocity in the particles of the body under consideration, and these motions may subsequently be changed into the molecular motions which we call heat, so that in the end more work comes to be transformed into heat than corresponds to the negative change of disgregation brought about. In many operations, especially in friction, the transformation of work into heat may take place even quite independently of any simultaneous negative transformation.

The relation in which the third kind of transformation, namely alteration of disgregation, stands to considerations of this nature, is implied in what has been already said. The positive alteration of disgregation may indeed be greater, but cannot be smaller, than the accompanying transformation of heat into work; and the negative alteration of disgregation may be smaller, but cannot be greater, than the transformation of work into heat.

Finally, in so far as regards the second kind of transformation, or the passage of heat between bodies of different temperatures, I have thought myself justified in assuming as a fundamental proposition what, according to all that we know of heat, must be regarded as self-evident, namely, that the passage from a lower to a higher temperature, which counts as a negative transformation, cannot take place of itself—that is, without a simultaneous positive transformation. On the other hand, the passage of heat in the contrary direction, from a higher to a lower temperature, may very well take place without a simultaneous negative transformation.

Taking these circumstances into consideration, we will now return once more to the consideration of the development by means of which we arrived at equation (II.) in § 5. Equation (2), which occurs in the same section, expresses the relation in which an infinitely small alteration of disgregation must stand to the work simultaneously performed by the heat, under the condition that the alteration takes place in a reversible manner. In case this last condition need not be fulfilled, the alteration of disgregation may be greater, provided it is positive, than the value calculated from the work; and if negative, it may be, when taken absolutely, smaller than that value, but in this case also it

would algebraically have to be stated as higher. Instead of equation (2), we must therefore write

$$dZ \geq \frac{AdL}{T}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2a)$$

Applying this to equation (1), we obtain, instead of equation (5),

$$\frac{dQ + dH}{T} + dZ \geq 0. \quad . \quad . \quad . \quad . \quad (5a)$$

The further question now arises, what influence would it have on the formulæ, if a direct passage of heat took place between parts of different temperature within the body in question.

In case the body is not of uniform temperature throughout, the differential expression occurring in equation (5a) must not be referred to the entire body, but only to a portion whose temperature may be considered as the same throughout; so that if the temperature of the body varies continuously, the number of parts must be assumed as infinite. In integrating, the expressions which apply to the separate parts may be united again to a single expression for the whole body, by extending the integral, not only to the alterations of one part, but to the alterations of all the parts. In forming this integral, we must now have regard to the passage of heat taking place between the different parts.

It must here be remarked that dQ is an element of the heat which the body under consideration gives up to, or absorbs from, an external body which serves only as a reservoir of heat, and that this element does not come into question now that we are discussing the passage of heat between the different parts of the body itself. This transfer of heat is mathematically expressed by a decrease in the quantity of heat H in one part, and an equivalent increase in another part; and accordingly we require to direct our attention only to the term $\frac{dH}{T}$ in the differential expression (5a). If we now suppose that the infinitely small quantity of heat dH leaves one part of the body whose temperature is T_1 , and passes into another part whose temperature is T_2 , there result the two following infinitely small terms,

$$-\frac{dH}{T_1} \text{ and } +\frac{dH}{T_2},$$

which must be contained in the integral; and since T_1 must be greater than T_2 , it follows that the positive term must in any case be greater than the negative term, and that consequently the algebraic sum of both is positive. The same thing applies equally to every other element of heat transferred from one part to another; and the alteration which the integral of the whole

differential expression occurring in (5a) undergoes, on account of this transfer of heat, can therefore only consist in the addition of a positive quantity to the value which would else have been obtained. But since, as results from equation (5a), the first value which would be obtained, without taking this direct transfer of heat into consideration, cannot be less than nothing, this can still less be the case when it has been increased by another positive quantity.

We may therefore write as a general expression, including all the circumstances which occur in non-reversible alterations, the following, instead of equation (II.):—

$$\int \frac{dQ + dH}{T} + \int dZ \geq 0. \quad . \quad . \quad . \quad (IIa)$$

The theorem which in § 1 was enunciated in reference to circular processes only, and was represented by the expression (Ia), has thus assumed a more general form, and may be enunciated thus:—

The algebraic sum of all the transformations occurring in any alteration of condition whatever can only be positive, or, as an extreme case, equal to nothing.

In my previous paper I have spoken of two transformations with opposite signs, which neutralize each other in the algebraic sum, as *compensating* transformations. The foregoing theorem may therefore be enunciated still more briefly as follows:—

Uncompensated transformations can only be positive.

§ 12. In conclusion, we will submit the integral

$$\int \frac{dH}{T},$$

which has been frequently used above, to a somewhat closer consideration. We will call this integral, when it is taken from any given initial condition to the condition actually existing, *the equivalent value of the heat in the body* (Körperwärme) *calculated from the given initial condition*. That is, when in any way whatever work is transformed into heat, or heat into work, and the quantity of heat present in the body thereby altered, the increment or decrement of this integral gives the equivalent value of the transformations which have taken place. Further, if transfers of heat take place between parts of different temperature within the body itself, or within a system of bodies, the equivalent value of these transfers of heat is likewise expressed by the increment or decrement of this integral, if it is extended to the whole system of bodies under consideration.

In order to be able actually to perform the integration which has been indicated, we must know the relation between the quan-

tity of heat H and the temperature T . If we call the mass of the body m , and its true specific heat c , we have, for an alteration of temperature throughout amounting to dT , the equation

$$dH = mcdT. \quad . \quad . \quad . \quad . \quad . \quad . \quad (23)$$

According to what has been said above, the true specific heat of a body is independent of the arrangement of its particles; and since a condition is known, namely, that of the perfect gases, for which we must regard it as established, partly by existing experimental data, and partly as the result of theoretical considerations, that the true specific heat is independent of temperature, we may assume the same thing for the other states of aggregation, and may regard the true specific heat as always *constant*. Thence it follows that the amount of heat present in a body is simply proportional to its absolute temperature, inasmuch as we can write

$$H = mcT. \quad . \quad . \quad . \quad . \quad . \quad . \quad (24)$$

The foregoing equation still remains applicable even when the body is not homogeneous, but consists of different substances, all, however, at the temperature T , if for c we substitute the corresponding mean value. On the other hand, if different parts of the body have different temperatures, we must in the first instance apply the equation to the separate parts, and then unite the various equations by summation. If, for the sake of generality, we assume that the temperature varies continuously, so that the body must be conceived as divided into an infinite number of parts, the equation takes the following form:

$$H = \int cT dm. \quad . \quad . \quad . \quad . \quad . \quad . \quad (25)$$

Applying this expression to the integral given above for the transformation-value of the heat in the body, and denoting the initial temperature by T_0 , we obtain, for the more simple case in which the temperature is uniform throughout,

$$\int \frac{dH}{T} = mc \int_{T_0}^T \frac{dT}{T} = mc \log \frac{T}{T_0}, \quad . \quad . \quad . \quad (26)$$

and, as a general expression embracing all cases,

$$\int \frac{dH}{T} = \int c \log \frac{T}{T_0} \cdot dm. \quad . \quad . \quad . \quad . \quad . \quad (27)$$

If the disgregation of a body is altered, without heat being supplied to or withdrawn from it, by an external object, the amount of heat contained in the body must be changed in consequence of the production or consumption of heat attendant on the alteration of disgregation, and a rise or fall of temperature

must be the result; consequently the question may be raised, How great must the alteration of disgregation be in order to bring about a given change of temperature, it being assumed that all alterations of condition take place reversibly? In this case we must apply equation (II.), putting $dQ=0$, whereby it is transformed into

$$\int \frac{dH}{T} + \int dZ = 0. 28)$$

If we assume, for the sake of simplicity, that the temperature of the entire body varies uniformly, so that T has the same value for all parts, we may apply equation (26) to the determination of the first of the two integrals; and we thus obtain, for the alteration of disgregation sought, the equation

$$Z - Z_0 = mc \log \frac{T_0}{T}. \quad (29)$$

If we desired to cool a body down to the absolute zero of temperature, the corresponding alteration of disgregation, as shown by the foregoing formula, in which we should then have $T=0$, would be infinitely great. Herein lies a chief argument for supposing it to be impossible to produce such a degree of cold, by any alteration of the condition of a body, as to arrive at the absolute zero.

XXX. *On the determination of Temperature and Pressure in Balloon Ascents.* By M. V. REGNAULT*.

[With a Plate.]

THE instruments usually employed to observe the temperatures and pressures in the various atmospheric strata which an aëronaut traverses, are difficult to observe and manage in the exceptional conditions in which the observer is placed. The mercurial barometer is fragile, its verticality doubtful, the mercurial column continually oscillates, and only one of its menisci can be observed. The aneroid is less fragile, the observation is easier; but its indications are uncertain for great variations of temperature, because the scale of the instrument often undergoes a derangement. Lastly, these instruments only give accurate indications when they are handled by a skilful experimenter—and only then, provided he is not distracted by the novelty of the spectacle offered to his view, and that he has not to occupy himself with the manœuvring of the balloon.

It appeared useful to me to seek for a method which might be practised with certainty by any aëronaut, and which required

* Translated from the *Annales de Chimie et de Physique*, February 1862.

neither observation nor numerical results to register. I think the method I am about briefly to describe satisfies all these conditions.

Fig. 4, Plate I. represents an air thermometer, consisting of a thin brass or silver tube ab , 20 centimetres long and 15 millimetres in diameter. It is closed at a , and terminates at b in a metallic capillary tube bc , provided with a stopcock r . Above this stopcock is fitted, by means of a screw-press (fig. 5), such as I often use in my apparatus, a short tube de , filled with fragments of chloride of calcium. At the top of the tube de there is a small aperture, so that the tube ab communicates freely with the external air when the stopcock r is open; but only perfectly dry air can penetrate.

Let us suppose twenty such tubes fixed on a small board, so that the reservoirs ab project beyond the board, and are in contact with the surrounding air; the capillary tubes being alone fitted on the board, and the stopcocks at the command of the aéronaut. These conditions are easily satisfied when the balloon is raised above terrestrial obstacles. The reservoirs of the air thermometers will not be influenced by the vicinity of the aéronaut; besides they can be removed from the boat as far as is desired, by making the capillary tubes sufficiently long.

The whole of these twenty tubes, which are all numbered, will form the *thermometric system*.

To avoid accidents in the descent, or in the carriage, it is better to arrange the tubes cylindrically as shown in fig. 6, the capillary tubes bc being simply fixed round a central axis, and the reservoirs ab remaining in free air. The system is then placed, for the sake of carriage, in a metal case, and is only fixed to the boat after the start.

The *barometric system* consists of twenty tubes like those of the thermometric system; but their reservoirs are immersed in melting ice during the whole balloon ascent. For this purpose a brass or sheet-iron bath, $ABCD$, fig. 7, is used in which the twenty reservoirs are arranged in parallel order in two rows, or round an axis as in fig. 5. This bath is filled with melting ice before the start. The water from the melted ice flows out by a large tubulure, tv . But it is necessary to preserve the ice from too rapidly melting at high temperatures, and especially from sinking below 0° in the low temperatures which prevail in the higher regions of the atmosphere. For that purpose the ice-bath $ABCD$ is placed in an annular bath, $EFGH$, filled with warm water at starting. The interval between the two baths is filled with a bad conductor of heat, such as cotton or charcoal-ashes. Under these conditions the balloon may travel several hours in regions where the temperature is -25° or -30° .

without the water in the water-case sinking below 0° . In any case, for greater safety, a bottle full of quicklime, or of caustic potash, may be taken; or indeed warm water, which can be used as ballast; and the temperature of the water in the case may be raised at will by introducing the substances by the tubulure O.

This ice-apparatus will be placed inside the boat; it occupies no great space, and its weight will be no hindrance for a balloon ascent; for it constitutes part of the ballast, which may be advantageously used to moderate the descent, by allowing the water to flow out of the water-case by the stopcock R.

Suppose the two systems conveniently arranged, all the stopcocks being open. The balloon is ascending; to determine at a given moment the temperature and pressure of any atmospheric layer, the observer closes *simultaneously* the stopcocks of the tubes No. 1 of the two systems. He will do the same at all the stations at which he desires to make an observation. To this simple operation all his work is limited.

The moment of the determination is not immaterial; the balloon ought not to be ascending or descending too rapidly; this is easily recognized by the motion of a sheet of paper attached by a thread to one of the cords of the boat. When the balloon ascends, the sheet sinks and sticks to the cordage; when it descends, on the contrary, the sheet rises, and the velocity of the descent is estimated by the amount of the divergence. The most favourable times for observing are those when the balloon is almost in equilibrium in the atmospheric layer, and makes oscillations, the existence of which is seen by the motion of the sheet of paper.

After the descent, the apparatus are brought to the laboratory of the physicist; and in a very short time he can accurately determine both the temperatures and the atmospheric pressures. For this purpose he uses a mercury manometer (fig. 9) like those which I frequently use for air-thermometers. The capillary tube bcd , in which the tube ab terminates, is provided with a steel socket d , which exactly fits by means of a screw-press (fig. 5) on each of the stopcocks r of the tubes of the barometric system, and of the thermometric system, used in the balloon ascent. On the tube ab are two marks, one at α , the other at β ; and by weighing the mercury which fills them, the capacities v and v' of the tube ab comprised between the levels α , β , and the end of the capillary tube bcd are exactly determined.

Let

x be the unknown temperature of the layer of air in which were the tubes No. 1 of the two systems at the time of closing.

y the barometric pressure, also unknown.

V the capacity at 0° of the tube No. 1 of the barometric system.

V' the capacity at 0° of the tube No. 1 of the thermometric system.

The mercury being brought to the level α of the tube ab under the pressure of the atmosphere H , a thermometer placed at the side of ab indicating the temperature t , the tube No. 1 of the barometric system is fitted on the manometer. This tube is surrounded by melting ice; mercury is allowed to flow out by the stopcock to expand conveniently the air of the space v ; it is thus seen whether the junction is tight; lastly, the stopcock of the tube No. 1 is opened. By pouring mercury into the manometer the level is brought to α , and the difference h of the two mercurial columns is measured. The elastic force of the air in the apparatus is $H-h$.

The weight of this air is expressed by

$$V \frac{H-h}{760} + v \frac{1}{1+at} \cdot \frac{H-h}{760}.$$

But this air is the volume v of air under the pressure H , and at the temperature t , which was in the manometer at the time of observation, and which is expressed by $v \frac{1}{1+at} \cdot \frac{H}{760}$. Further, of the volume V of air under the pressure y which filled the tube No. 1 at the time of closing the stopcock; the weight of this air is $V \frac{y}{760}$. We have thus

$$V \cdot y + v \frac{1}{1+at} H = V(H-h) + v \frac{1}{1+at} H-h,$$

whence

$$y = H-h - \frac{v}{V} \frac{1}{1+at} h.$$

This equation will give the unknown barometric pressure y ; but $\frac{v}{V}$ must be known. It is determined directly in the apparatus just as it is arranged. Mercury is allowed to flow out of the manometer until its level is at C ; the difference of the two mercurial columns is then h' , the elastic force of the air is $H-h'$, and we ought to have

$$V \frac{H-h}{760} + v \frac{1}{1+at} \frac{H-h}{760} = V \frac{H-h'}{760} + v' \frac{1}{1+at} \cdot \frac{H-h'}{760},$$

or simply

$$V(h-h') = \frac{1}{1+\alpha t} [v(H-h) - v'(H-h')].$$

V is readily deduced from this, and hence also $\frac{v}{V}$.

The tube No. 1 of the thermometric system is treated in just the same way; we shall get for it

$$V' \frac{H'-h''}{760} + v \frac{1}{1+\alpha t'} \frac{H'-h''}{760} = V' \frac{(1+kx)}{1+\alpha x} \frac{y}{760} + v \frac{1}{1+\alpha t} \frac{H'}{760},$$

whence

$$\frac{1+kx}{1+\alpha x} = \frac{1}{y} \left(H'-h'' - \frac{v}{V'} \frac{1}{1+\alpha t'} h'' \right);$$

y is known by the experiment with the tube No. 1 of the barometric system, and $\frac{v}{V'}$ is determined in the apparatus in the same manner as we determined $\frac{v}{V}$.

Thus x is deduced from the preceding equation. I am convinced that in this manner temperatures and pressures would be obtained with greater precision than by the direct observations of the ordinary instruments. The aëronaut has only to attend to the simultaneous closing of the stopcocks of both tubes of the same number in the two systems; he does not influence the results.

It must be ascertained before starting that the tubes are exhausted; that is easy by means of the manometer, fig. 9. Glass tubes may be employed instead of metal; but their fragility is to be feared.

The thermometric tubes with a metallic envelope are easy to construct, and I have used them frequently for low and for high temperatures. A change of capacity in consequence of a variation in the pressure is not to be feared: thus an air thermometer exactly like those I propose for balloon ascents was surrounded by melting ice; the levels of the mercury in the tubes of the manometer were read off on the cathetometer. These levels did not change when a complete vacuum was made in the cylinder in which the ice was placed; and yet the sides of the tube ab were not a quarter of a millimetre thick.

It might be feared that these thermometers are not sufficiently sensitive to slow and continued variations of temperature; but they are clearly more so than mercurial thermometers, however small their reservoirs may be. I say, further, that they are as sensitive as metallic thermometers, at any rate if these are not formed of very thin, spiral, compensating plates; but in this case these instruments are so mobile that they can only be used

in a state of perfect quiescence. Finally, metallic thermometers never give certain indications. Their scales are continually displaced by the same causes which determine the displacement of zero for the mercurial thermometer; but the displacement of zero, which does not exceed a few tenths of a degree in the mercurial thermometer, may reach several degrees in the metallic thermometer.

I will say, in conclusion, that it is especially interesting to know the law which the temperature follows in the various atmospheric layers *during the night*; thus a balloon ascent which had this object ought to be made during a calm night by the light of the moon; and the method I propose is then quite applicable.

XXXI. *On the Colour of Water.* By W. BEETZ*.

[With a Plate.]

IT is only in recent times that explanations based upon actual experiments have been given of the colour of water in the sea, in lakes, and in rivers; it was previously thought sufficient to conceal the entire ignorance of a daily-observed phenomenon by hypothesis. Bunsen† was the first to state, and establish experimentally, the simple proposition that “chemically pure water is not, as commonly assumed, colourless, but naturally possesses a blue colour.” He observed this coloration on looking at a piece of white porcelain through a column of water two yards long. He explained the brown to black coloration of many waters, especially of North German inland lakes, as arising from an admixture of humus; the green colour of the Swiss lakes, and, still more so, the siliceous springs of Iceland, as arising from the colour of the yellowish base, and of the siliceous sinter surrounding the springs, and which is caused by traces of hydrated oxide of iron. Wittstein‡, by careful chemical investigations, has quite recently shown that the green colour also derives its origin from organic admixtures. According to him, the less organic substance a water contains, the less does its colour differ from blue. With the increase of organic substances, the blue gradually passes into green, and from this, as the blue is more and more displaced, into brown. Water is softer the nearer it is to brown, and harder the nearer it is to blue; this does not arise from a greater or less quantity of organic substance, but of alkali, on which, again, the proportion of dissolved organic substance depends. This alkali dissolves the

* Translated from Poggendorff's *Annalen*, January 1862.

† Liebig's *Annalen*, vol. lxii. p. 44.

‡ *Sitzungsber. der K. bayer. Akad. der Wissensch. in München*, 1860, p. 603.

organic substance in the form of humic acid. If a water does not contain much humic acid, this is not caused by a want of humic acid in the ground, but by the fact that this ground did not give to the water an adequate quantity of alkaline solvent material.

From these results we may consider the question settled as to why, on chemical principles, some waters are blue, others green, and others brown. I may be permitted to make a few remarks on some physical phenomena which have been observed on coloured waters.

Formerly water was almost universally classed among those bodies which have a different colour in transmitted, to that which they have in reflected light. Newton says*, "water reflects the violet, blue, and green rays, but readily transmits the red." Count Xavier de Maistre† considers the colour of water to be blue in reflected, and yellowish orange in transmitted light. Arago‡, that it is blue in reflected, and green in transmitted light. The view that the blue of water only occurs in reflected light is common to all three statements.

In the experiments which Bunsen made to ascertain the colour of distilled water, transmitted light was alone concerned, and yet he found the colour blue. In order to look through still longer columns of water I used the following apparatus:—A box, the bottom and sides of which ($a a'$, fig. 2, Plate I.) are made of plates of gutta percha, is closed at both ends by parallel plates of very white thin plate glass, $b b'$. Directly inside these, two similar glass plates are fixed, which are covered with a silver reflecting surface, by Liebig's method. A narrow slit is scratched in the covering at d and d' , as seen in fig. 3, Plate I. If a pencil of direct sunlight is projected upon slit d , this will be reflected several times backward and forward between the two mirrors; if the box is filled with a liquid, the light is compelled to traverse this liquid repeatedly, and it is easy to increase or diminish the length of the layer to be traversed, by altering the angle of incidence. This experiment may be made either objectively or subjectively. If the pencil is allowed to fall into the slit d , so that after a certain even number of reflexions it falls directly upon the slit d' , it can be caught upon a screen after its emergence. The number of reflexions may be altered by gradually rotating the box. But if the observer uses the illuminated slit d as a self-luminous object, and looks through d' into the box, he sees, close to one another, a series of narrow subjective pictures of the slit; they are gradually smaller and nearer each

* *Optices*, lib. i. pars 11. Prop. 10. Exp. 17.

† *Salmonia*, 3 ed. p. 317; *Pogg. Ann.* 1st Supplement, p. 67.

‡ *Comptes Rendus*, vii. 219; *Pogg. Ann.* vol. xlv. p. 470.

other, and correspond to the different numbers of reflexions. In making some experiments, I had, at first, so placed the mirrors that the uncovered glass surfaces were opposite each other. The light must then, at each reflexion, pass twice through the glass plates themselves. If the liquid contained no liquid, then the image appeared almost white after six to eight reflexions; but still, on comparing the subjective images with one another, it could be seen that each following one had a somewhat yellowish tint. I supposed that this coloration was to be ascribed to the tolerably thick layer of glass which the light had to traverse, and therefore turned the mirrors, which were once more polished on the silvered side. Yet even in this case each following image showed a yellower colour, though in a less degree. The colour must therefore be ascribed to the special colour of the silver, from which part of the light is reflected diffusely. Yet when the polish is good, it is so unimportant that it does not disturb further observation.

If the box is half filled with distilled water and the entire slit d illuminated, the lower part of the picture on the receiving plate is seen to be blue, while the upper part remains white. Looking through the slit d' at the upper part of the box, the entire range of more and more yellowish pictures is seen; looking through the lower part, each following picture appears of a darker blue, with a very feeble tinge of green. The phenomenon is just the same when water from the deep blue Achensee is poured into the vessel; if this is replaced by water from the Tegernsee, after a few reflexions the images appear of an intense yellowish green (not bluish green), although my box was only 10 inches long. If garden earth is drenched with water, which is allowed to drain off, and this is filtered and mixed, first in small and then in larger quantities, with distilled water, the colour of the images passes first into yellowish green and then steadily into a brown colour, just as was to be expected from Wittstein's experiments. The colours in question in these experiments are also those in transmitted light.

What, then, are the phenomena which have evoked the idea of the dual colour of water?

Newton based his view upon an experiment of Halley. As the latter, on a sunny day, had descended in a diving-bell to a great depth in the water, the upper surface of his hand, which was directly illuminated through the sea-water and through a window in the bell, appeared of a rose-red, but the water below him and the under surface of his hand, which was illuminated by the rays reflected from the lower water, was green. The experiment is manifestly erroneously interpreted. The rays which came from below are clearly not reflected by the water, but trans-

mitted; they are reflected indeed from foreign substances in the water, especially from the sea-bottom. The further distant this is, that is, the deeper the sea at the given place, the deeper will be the colour of the water—deep green when the water has a green, deep blue when it is blue (in transmitted light). The rays which fell from above into the bell must also show the colour of water, but to a much smaller extent, because the layer of water which they traverse is, in any case, much less thick than that which the rays coming from below have traversed. Thus the upper rays brought comparatively more white light than the lower ones; and hence the upper surface of the hand had the complementary colour, that is, rose-red, for the same reason for which, in the blue grotto at Capri, the contrast colour, orange, occurs.

Arago adduces no experiment in support of his view; he only proposes to make one, to which reference will afterwards be made. He introduces his view with the words “the reflected colour of water is blue, the transmitted, as some think, green;” and upon this supposition he bases the explanation of some phenomena. He shows, in particular, why the waves of the blue sea are green. He considers them as water prisms, on one surface of which the white daylight is reflected, sent through the following wave, and thereby made green. But it is easy to see that in the green waves, as well as in the large blue mass of water, it is only a question of transmitted light. On looking at the mirror-like surface of the Achensee in a perfect calm, the colour is seen to change from a deep blue in the middle to a bright green, and thence into a yellowish red. This water, which contains very small quantities of humus salts, colours the light greenish when it only passes through thin layers, and blue when it passes through thicker. This phenomenon has many analogies. Newton says*, it must be noticed that in coloured liquids the colour alters with the thickness. For instance, a red liquid in a conical glass held between the light and the eye appears pale yellow near the bottom, where it is thinnest, somewhat higher, where it is thicker, of a golden yellow, where it is still thicker, red, and where it is thickest, dark red. Hence it must be assumed that such a liquid absorbs the violet and indigo rays very readily, the blue rays with greater difficulty, the green ones with still greater difficulty, and the red ones most of all.

This is just the case with bluish-green sea-water. It absorbs the red rays very easily, the green ones with more difficulty, and the blue ones most of all. Hence when white daylight passes to the bottom through a thin layer of this water, and reflected from this returns to the air, it is feebly green. If on both

* *Loc. cit.*

courses it has traversed great distances, it is blue. It also appears green when it has passed through the moderately thin section of a wave (which it may indeed have reached by reflexion from another wave).

I spoke just now of the reddish-yellow colour in the almost dry places which has been noticed by so many observers. This colour depends entirely on the nature of the ground. Most frequently it consists of whitish sand, or whitish pebbles. If these were absolutely white, if they reflected colours in a diffused manner to the same extent, the reddish colour would not occur. A new porous clay-cell of a Grove's battery may appear quite white, while, when it is moistened with water, it is of a rusty yellow or flesh-red colour. Hence its surface acquires the property of reflecting red light to a preponderating extent. If, now, the substances which constitute the sea-bottom have the same property, the bottom will appear red in those parts in which it is covered with quite thin layers of water. If the thickness of the layer of water increases, fewer red rays reach the bottom; the returning rays are again partially absorbed by the water, and thus the red colour is continually disappearing, although the forms of substances lying on the ground can always be distinctly perceived.

Moreover, this red colour is much increased by contrast. In the dry places of the Aar I have often observed that the bright red, which they show, diminishes considerably when they are viewed, not near the beautiful green of the deeper water, but through an isolated tube.

There might seem to be a fact in disaccordance with the statement, that sea-water in thin layers is green, and blue in thicker; a white object, for instance an oar, appears of a distinctly pure blue when immersed at even a very inconsiderable depth below the level of the Achensee, while it is of an intense green below the Tegern- or Königsee. The light which impinges upon the white surface of the oar, has had to traverse a much more considerable distance than that from the surface of the water; it comes from the side through a considerable mass of water, in which it has assumed the characteristic colour of the lake. But if the same white surface is brought near the bank and turned towards it, and is at the same depth as in the former case, it is seen in the Achensee to be of an almost unaltered white, while in the Tegernsee it is always somewhat greenish; for the colour of the blue water is only perceptible at great distances, that of the green at very small ones.

This surprising strong coloration in consequence of laterally incident light, led me to the proposal which Arago has made, to investigate the true colour of water in transmitted light.

A hollow prism made of glass plates is so brought under water that the horizontal light from the surface is totally reflected from the hypotenuse. Instead of this apparatus, Poggendorff* proposed a glass mirror inclined at 45° to the horizon. I happened to have occasion to make a corresponding experiment; I wanted to fill a tinplate tube, which was closed at the ends with glass plates and had a hole in the side, by placing it in a very inclined position quite under the sea-level. When the upper glass plate had the right inclination, in the Tegernsee it reflected in sunny weather an emerald-green light more intense than I have obtained in any other way; in the Achensee, however, a blue light, as if it had passed through concentrated solution of sulphate of copper. Hence Arago's proposal is appropriate; and if he had had an opportunity of carrying it out, he would certainly have given up the notion that water shows different colours in reflected and in transmitted light.

The colour of water alters naturally, when solid particles are suspended in it. By mixing such bodies which, like the above-mentioned constituents of the ground, reflect red light in preference when they are moistened, it may yet appear red; by greater masses of whitish sand which have become heaped up in the lakes during a continuous storm, or which the rivers have worn down from their beds, the water appears clearer than otherwise. Simony† observes that the Wolfgang and Attersee appear in winter, when they are clearest, of a dark green, but in summer bluish-green or cerulean blue, and he considers this colour as occasioned more especially by the marl and grey sandstone predominant in the débris.

In the previous considerations, the influence of the colour of the sky and of the surrounding neighbourhood has been disregarded. Yet there are many who seek the cause of the colour of water in these circumstances. But these secondary influences must be taken into account along with the chief cause. When the surface of the lake is quite clear, it acts as a mirror. The special phenomena of colour are the more concealed, the more regular reflected light reaches the eye from the place in question; they appear purest where no or but little light is regularly reflected, for example, against a dark rocky background. But if the sea is in motion, the regular reflexion always diminishes, and the aspect of the surface is changed by the occurrence of waves in a very complicated manner, depending on the formation of the bank, the direction and intensity of the wind, and similar

* Poggendorff's *Annalen*, vol. xlv. p. 474.

† Wiener *Sitzungsbericht*, iv. 542.

circumstances, which the mariner can recognize, and even predict from that aspect.

I permit myself a remark as to the place in which the green colour of water arises. The Tegernsee receives its water by several supplies, among which the Weissach and the Rottach are the most considerable. After lengthened dry weather, the bed of the Weissach is quite empty; the pebbles which cover the bottom are quite dry, and almost white. After a time of such weather, I went up the course of the Weissach in order to observe the first water which moistened the ground. This water could have no other origin than the atmosphere. Yet the first quantity, which was sufficient to look through in bending over the bed, immediately appeared green. Hence the humic acid salts must have been already formed in the bed of the river, and are only dissolved by the water; it is not necessary to assume that the springs which fed the rivers must bring an alkaline solution which shall afterwards dissolve the humic acid.

Water, of atmospheric origin, in its purest condition of ice and snow is also blue. The glaciers of the Alps and of Iceland also show this colour* when the adjacent waters, which in part arise from the glacier streams, are green. H. and A. von Schlagintweit† estimate the colour of glacier-ice in the crevasses as being equal to the mixed colour shown by a colour circle on which 74·9 parts of white, 24·3 of cobalt blue, and 0·8 part of green were painted. Osann‡ saw that the light in a hole in the mountain snow about two feet deep was blue, and believed that this colour was due to the blue colour of the air, which has a deeper blue in the upper than in the under layers, and he therefore thinks that the blue colour of glacier-ice is heightened by that of the air in those higher regions. But the experiment on which he depends succeeds with freshly fallen snow on the plain as well as above the snow-line. The other colour depends on the colour of the small crystals of ice which the light repeatedly reflected backwards and forwards in such a small hole must traverse. Green ice can only be caused by the freezing of green lakes and rivers; the atmospheric fall, and the compression of the high snow can only give rise to the formation of blue ice.

Erlangen, December 1861.

* Bunsen, *loc. cit.* p. 47.

† *Phys. Geogr. der Alpen*, i. page 22.

‡ *Verh. d. Würzb. Ges.*, iv. 231.

XXXII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 162.]

June 20, 1861.—Major-General Sabine, R.A., Treasurer and Vice-President, in the Chair.

THE following communications were read:—

“On the Heat which is developed at the Poles of a Voltaic Battery during the passage of Luminous Discharges in Air and in Vacuo.” By John P. Gassiot, F.R.S.

1. When the wires attached to the terminal plates of an extended series of a voltaic battery are brought into contact with each other, the circuit of the battery is completed; and if in this state the ends of the wires are separated from each other, the usual luminous or arc-discharge is produced, the length of the arc depending on the number of the cells of which the battery consists. If this luminous discharge is continued for a few seconds, the metallic positive pole or anode becomes red-hot, and will ultimately be fused, while the negative remains comparatively cool. This experiment was originally described by me in the ‘*Philosophical Magazine*’ of December 1838, p. 436. In the same periodical of June 1840, p. 478, Mr. Grove suggested as an explanation, that this effect “might be due to the interposed medium, and that, were there any analogy between the state assumed by voltaic electrodes in elastic media and that which they assumed in electrolytes, it would follow that the chemical action in the positive electrode in atmospheric air would be more violent than at the negative, and that, if the chemical action were more violent, the heat would necessarily be more intense.”

2. Since that time I am not aware that any other explanation relative to the heating of the positive pole of the voltaic battery has been published. Mr. Grove merely gives it as a suggestion; but as it is immaterial whether either or both poles are of copper, aluminium, platinum, or any other metal, or of coke, as in either case it is the positive that is heated, the phenomenon cannot arise from any effect of oxidation, but must be due to some action in the battery circuit hitherto unexplained.

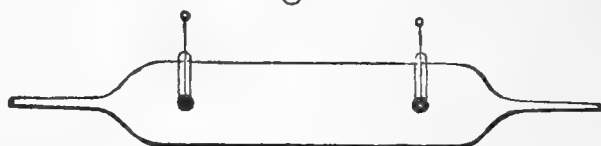
3. In the Bakerian Lecture for 1858, I have stated that, “when the discharge from an induction coil is taken in air or in vacuo with thin platinum wires, the negative terminal becomes red-hot, and if the discharges are continued the wire will be fused.” This heating of the negative terminal, provided the wires are thin, always takes place whatever may be the length of the discharge or the medium through which it passes.

4. That this heating of the negative terminal in the discharge from an induction coil had some intimate relation to the heating of the positive pole of the voltaic battery was very probable; but why in the one case the heat should be evolved at the negative, and in the other at the positive terminal, appeared extraordinary and well

worth further investigation. The result of the experiments I have made with this object forms the subject of the present communication.

5. After verifying the fact that the heating of the negative terminal of an induction coil is always obtained either in a carbonic acid vacuum, in rarefied gases, or in air, provided the terminal wire is thin, but not if thick, I proceeded to examine with greater precision than formerly the nature and character of the luminous discharges in vacuo as obtained from my water-battery, as well as from the 400 insulated cells of the nitric-acid battery (Phil. Mag. S. 4. vol. xx. p. 540); and for this object I had several small vacuum-tubes constructed, about 3 inches long and 1 inch diameter; in each of these, two metallic or carbon balls, about $\frac{1}{8}$ of an inch in diameter, were attached to the platinum wires, hermetically sealed in the tube about one inch apart.

Fig. 1.



Each wire is protected by a glass tube as far as the ball; the vacuum is obtained by means of carbonic acid absorbed by caustic potassa, as described in my former communications.

6. The discharges in these tubes from an induction coil or from my water-battery present nearly the same appearance, viz. a brilliant luminosity surrounding the negative ball, generally without (fig. 2), but sometimes with (fig. 3) a minute stratified discharge from the positive.

Fig. 2.



Fig. 3.



7. When the vacuum-tube is introduced into the circuit of 400 insulated cells of the nitric-acid battery, the discharge at first always assumes the form represented in fig. 2. This discharge, as well as those from the induction coil and the water-battery, when examined, is found to be very perceptibly intermittent, and will generally continue for some time after the circuit has been completed. As the action of the battery improves, *the luminous glow round the negative metallic ball gradually increases in size*, and in a few seconds the ball becomes red-hot. This result I repeatedly obtained; and in two instances with tubes in which balls of aluminium, $\frac{1}{4}$ inch diameter, had been

Fig. 4.



inserted, the negative dropped from the wire into the tube in a molten state, but leaving the positive ball with its original metallic lustre.

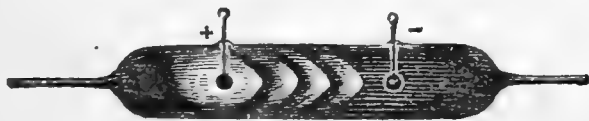
8. The preceding experiments, so far from assisting me in explaining the cause of the heating of the positive pole of the voltaic battery, rather tended to complicate the inquiry ; for experiment now showed that in carbonic acid vacua the heating effect was elicited at the negative pole, whether the discharge was made from a voltaic battery or from an induction coil ; but as experiment also showed that heat is always evolved from the latter at the negative, whether in air or in vacuo, and that from the battery it always in air appears at the positive, it was evident the media through which discharges were made afforded no explanation for the elucidation of a phenomenon of which hitherto the more it was examined by experiment the more difficult appeared the solution.

9. My next experiments were made in vacuum-tubes in which balls of carbon were inserted in lieu of metal. With these I at first obtained the usual heating of the negative, but in one instance I observed that both balls were at the same time exhibiting red heat ; in this instance, either from some alteration having taken place in the vacuum, or from some other cause which I had not time to examine, the discharge from the carbon balls became so uncertain as to afford me little information worth recording, except as to the fact I have stated of both balls being heated : the cause of this I was subsequently enabled to determine.

10. In two of the vacuum-tubes hollow brass balls had been attached to the platinum wires. In the first of these the negative very soon became heated by the discharge. As the discharge of the battery continued, a sudden flash of light was visible in the vacuum, and the glass instantly became coated with metal. On examining the tube, I ascertained that one-half of the negative ball was separated from the other, and partly fused : the intense heat had vaporized the silver with which the two hemispheres forming the ball had been soldered, and it was this vaporized metal that was deposited on the sides of the tube. The positive ball remained much tarnished, as if it had also been heated.

11. I then introduced the second tube, having similar hollow brass balls, into the circuit of the battery. In this tube the negative also was at first heated to redness ; when in this state, a sudden brilliant stratified discharge took place, as fig. 5.

Fig. 5.



The evolution of nitrous acid fumes in the battery denoted its intense action. This discharge from the battery continued for about two seconds, *the negative ball instantly losing its luminosity, the positive becoming red-hot.*

12. This experiment appeared to offer an explanation as to the cause of the heating of the poles ; but on attempting to repeat it, the tube was unfortunately fractured by the heating of the platinum wire, as were also two tubes with coke balls : the result I had obtained was,

however, too interesting to me not to repeat the experiment; and I had six vacuum-tubes prepared, each of the same dimensions as already described, two with coke balls, two with iron, and two with copper, all solid: the best and most conclusive results were obtained with the copper. The experiments which I now proceed to describe were made on the 27th of last May, in the presence of the Rev. Dr. Robinson and Professor Stokes.

13. In the first tube the negative ball very soon became red-hot, when suddenly the brilliant, clearly defined, conical stratified discharge as in fig. 5 appeared, continuing for three or four seconds. On breaking the circuit of the battery, we ascertained that the negative ball, or that attached to the zinc terminal of the battery, had, as before, entirely lost its heated appearance, while the positive, or that attached to the platinum end of the battery, remained at a dull red heat, which it retained for two or three seconds after the circuit of the battery had been broken.

14. A second tube with copper balls was then introduced into the battery circuit; in this I also obtained the usual luminous negative glow discharge surrounding the ball attached to the zinc terminal of the battery; this glow, as before, gradually enlarged, and in a few seconds the ball became red-hot; the circuit of the battery was then instantly broken, the negative ball retaining its red heat for several seconds. In this experiment it will be observed that the circuit of the battery was broken before the sudden brilliant discharge, previously described, had appeared. These results fully confirmed my original experiment (11) of the alternate heating of the positive and negative terminals of the voltaic battery, when the discharge is made in vacuo.

15. The result of my previous experiment (11) having been thus confirmed, the explanation of those that preceded it became easy of solution. When both coke terminals (9) were heated to redness, the continuous or arc discharge had only lasted for an instant, evolving heat at the positive terminal; but the negative during this short interval retained the luminosity it had previously acquired, and consequently at the time of the disruption of the circuit, both coke terminal balls exhibited red heat. Again, in the first experiment with the brass balls (10), the arc discharge was also only momentary, a disruption of the circuit having been caused by the partial fusion of the negative ball; but the duration of this discharge was sufficient to tarnish the positive terminal; while with balls of aluminium (7), a metal that fuses at a very low temperature, the negative was melted by the heat evolved in the intermittent discharge; and we ascertain that under this condition the positive terminal is in no way affected, for the metal retained its original lustre.

16. The voltaic discharge of a battery of 400 insulated cells, charged with nitric and sulphuric acid, when observed in these vacuum-tubes, at first assumes precisely the same appearance as that of one of nearly 4000 cells charged with rain-water, as it is also similar to that of the discharge from an induction coil (fig. 2); but the action of the nitric acid so far differs from that of the water-battery, that while the discharge of the latter will remain for several weeks (or until the

water in some of the cells has evaporated) of nearly the same form and appearance (figs. 2 or 3), the discharge from the nitric-acid battery quickly alters : as the action of the battery improves, the glow round the negative terminal enlarges, heat in that portion of the tube is sensibly developed, as can be ascertained by touching the tube with the hand, and in a few seconds the negative ball becomes red-hot. During this time no apparent sign of chemical action takes place in the cells of the battery ; the heating effect appears to be mainly due to the elevation of tension ; for if the copper wires attached to the terminals rest on gold-leaf electrosopes, the leaves continue expanded until the arc discharge takes place, when they instantly collapse, and heat is evolved at the positive terminal.

17. In vacuo, as long as the intermittent discharge continues, resistance apparently takes place at the negative terminal ; and this is not only evinced by the heat which is evolved, but by the disruption of the metal which is separated from this pole in minute particles, and deposited in a lateral direction on the sides of the vacuum-tube.

18. As soon as the action of the battery becomes sufficiently energetic, so as to cause the continuous or arc discharge to pass, an entirely new effect is developed : the discharge itself becomes intensely vivid, the stratifications assume a conical form with their apex directed towards the negative (fig. 5), and heat is instantly perceptible at the positive terminal, while the negative as instantly cools.

19. It is beautiful and interesting to observe the suddenness with which the red heat of the negative terminal ball disappears, and the equal suddenness with which the heat is at the same instant elicited in the positive, when the brilliant arc discharge takes place.

From the results obtained by these experiments, I infer that the development of heat, either at the positive or the negative pole of a voltaic battery, is entirely due to the amount of resistance which takes place in that part of the battery circuit.

“Contributions to the Physiology of the Liver.—The Influence of an Acid in producing Saccharine Urine.” By F. W. Pavy, M.D.

“On the Chemical and Physical Conditions of the Culture of Cotton.” By J. W. Mallet, Ph.D., F.C.S. &c.

“Accounts of Experiments made at Holyhead (North Wales) upon the Transit-Velocity of Waves through the Local Rock Formations.” By Robert Mallet, Esq., C.E., F.R.S.

These experiments were made by the author at the joint request of the Royal Society and of the British Association for the Advancement of Science, aided by grants from each of those bodies.

Their object was to ascertain the transit rate or velocity of propagation of waves of elastic compression, analogous to those of natural earthquakes, through the stratified and highly convoluted, laminated, and shattered slate and quartz rocks of the neighbourhood of Holyhead, where the recurrent explosion of very large mines of gunpowder in the Government quarries (for the obtaining of rock for construction of the Asylum Harbour) afforded a valuable opportu-

nity in the production of the requisite impulses for generation of the wave.

The author had previously determined experimentally (at Killiney Bay, in Ireland) the transit velocity of such waves in wet sand, and in highly-shattered and more solid granite; media presenting, probably, the extremes of slowness and of fastness of wave transit. It was still desirable to determine this for rock, not only minutely crystalline, but also stratified, convoluted, and generally highly perplexed and heterogeneous in internal structure. The instrumental means employed were generally similar to those adopted previously at Killiney Bay, with suitable modifications consequent upon the great charges of powder fired, which at these quarries have reached as much as nine tons at a single blast or mine. The seismoscope (see *Trans. Brit. Assoc.* 1851, *Second Report on Facts of Earthquakes*, R. Mallet, p. 277, &c.) was placed upon a levelled table of solid rock at a suitable station (shown on the map and sections that accompany the paper), and with it the chronograph and galvanic apparatus, by which, on making contact, by the author's pressing his hand upon the lever of the latter instrument, the mine at the quarry, distant in all cases about a mile, was ignited, and the time that elapsed between the starting of the elastic wave from the impulse of the explosion to its arrival at the observer and visibility in the field of the seismoscope was recorded. This registered time was subject to three principal corrections, the respective coefficients of which are also determined experimentally. The instruments admitted of time determinations to within nearly $\frac{7}{1000}$ of a second. The range over which the wave traversed was accurately obtained in length for each separate experiment. A constant distance from the observing station = 4584·80 feet, up to a fixed point near the quarries, was obtained with precision, in the first instance, by trigonometrical operations, upon a measured base of 1432 feet. The distance of the mean centre of each mine or heading was subsequently measured in a right line to this fixed point, and the angle made by the latter with the former line determined, whence the direct distance between the mean centre of each particular "heading," or mine, and the observer's station, was trigonometrically deduced.

The following Table gives part of the results obtained from six good experiments.

TABLE.

No. of experiments.	Weight of powder exploded.	Total distance of mean centre of heading from observer.	Total observed time of transit.	Observed rate of transit per second, uncorrected.	Final corrected transit rates observed.
	lbs.	feet.	seconds.	feet per second.	feet per second.
1	3,200	6582·93	7·346	896·12	1016·200
2	2,100	5476·57	5·658	967·93	1098·958
3	2,600	6377·14	6·524	977·26	1109·483
4	6,200	6403·48	5·455	1173·87	1331·168
5	12,000	5038·13	4·161	1210·79	1373·035
6	4,400	5228·59	5·249	996·11	1129·598

One very remarkable result is at once apparent on inspecting this table—viz. that the transit rate tends to increase in velocity with the increased quantity of powder fired; in other words, that the loss of velocity in the same rock is less in some proportion as the force of the originating impulse is greater, and so its amplitude greater at starting. This is seen if the experiments be arranged in the order of increased weight of powder.

No. of experiment	2.	3.	1.	6.	4.	5.
Weight of powder	2100 lbs.	2600 lbs.	3200 lbs.	4400 lbs.	6200 lbs.	12,000 lb.
Uncorrected transit velocities . .	967·93	977·26	896·12	996·11	1173·87	1210·79

Experiment No. 1 forms the only apparent exception, and even there the departure is not large.

This fact, now for the first time (so far as the author knows) experimentally proved, appears remarkably in coincidence with the theoretical researches of Mr. Earnshaw.

The general mean transit velocity derivable from all the experiments taken together gives 1176·407 feet per second for the rate. The results, however, obviously form two groups—viz. Nos. 1, 2, 3 and 6 from the smaller, and Nos. 4 and 5 from the greater charges of powder. The mean from the first four is 1088·5597 feet per second; that from the two last 1352·1015 feet per second; and taking a mean of means from both, we obtain 1220·3306 feet per second as the mean transit velocity of propagation, in the rocks experimented on, of wave pulses due to the impulse of explosions of not exceeding 12,000 lbs. of powder.

The first mean from the smallest charges is that which must be compared with the Killiney Bay experiments. It thus appears that the wave velocity in highly contorted and foliated rock is very low, and is intermediate between the transit rate in wet sand and in discontinuous granite, or

In wet sand = 824·915 feet per second.

In contorted and stratified rock, quartz, and slate = 1088·559 feet per second.

In discontinuous granite = 1306·425 feet per second.

In more solid granite = 1664·574 feet per second.

The general mean obtained, 1220·33 feet per second, or 13·877 statute miles English per minute, co-ordinates, as might be expected, with the carefully made deductions of Nöggerath and of Schmidt from the actual earthquakes of the Rhine and of Hungary, as well as with those of the author from the great Naples earthquake of 1857.

In experimenting with these great explosions the author was enabled to observe, by means of the seismoscope, that the advent of the great wave of impulse (which was sometimes sufficient to make the mercury sway visibly in the trough of the instrument) was preceded by rapidly augmenting tremors, quite like those which very generally precede the great shock in natural earthquakes.

The wave transit in these experiments was made partly in slate rocks and partly in quartz formations, which, though lithologically and geologically distinct, are nearly identical in wave propagative power (as this author has shown by a train of special experiments at the conclusion of the paper), differing not more than in the ratio of 0·576 for the slate to 0·558 for the quartz. The author concludes by pointing out several deductions having interest to general physics, and some of the special relations of the results to Seismology and Physical Geology.

“On the Cutaneous Sensibility of the Hand and Foot in different parts of the Surface, as tested by the Continuous Galvanic Current.” By Harry Lobb, Esq.

“Experiments and Observations on the Structure and Function of the Stomach in the Vertebrate Class.” By William Brinton, M.D.

“Notes of Researches on the Intimate Structure of the Brain.”—Second Series. By J. Lockhart Clarke, Esq., F.R.S.

“On the Influence of Atmospheric Pressure upon some of the Phenomena of Combustion.” By Dr. Edward Frankland, F.R.S.

The author has concluded his experiments upon this subject; and, in addition to the details of the results which have already been briefly mentioned in the *Philosophical Magazine*, S. 4. vol. xxii. p. 549, communicates the following :—

Although *the rate of burning* of candles and other similar combustibles, whose flames depend upon the volatilization and ignition of combustible matter in contact with atmospheric air, is not perceptibly affected by the pressure of the supporting medium, yet this is not true of all combustibles. The rate of burning of self-supporting combustibles, like the time-fuses of shells, depends essentially upon the pressure of the medium in which they are deflagrated. Attention was first called to this fact by Quartermaster Mitchell*, who found that the fuses of shells burnt longer at elevated stations than when ignited near the level of the sea. The results of the author's experiments with six-inch or thirty-seconds fuses burnt in artificially rarefied air are embodied in the following Table :—

Average pressure of air in inches of mercury.	Average time of deflagration of six-inch fuse.	Increase of time of burning over preceding observation.	Reduction of pressure corresponding with increase of time.	Increase of time for each diminution of one inch pressure.
	Seconds.	Seconds.	Inches.	Seconds.
30·40	30·33			
28·25	32·25	1·92	2·15	·893
25·70	34·75	2·50	2·55	·980
22·45	37·75	3·00	3·25	·925
19·65	41·50	3·75	2·80	1·339
15·95	45·50	4·00	3·70	1·081

* Phil. Mag. S. 4. vol. x. p. 48.

There are here evident indications of the rate of retardation being somewhat greater at low than at comparatively high pressures ; but, neglecting these indications, the above numbers give 1·043 second as the average retardation in a six-inch or thirty-seconds fuse for each inch of mercurial pressure removed. This result agrees closely with that obtained by Quartermaster Mitchell, if we except those fuses which he burnt at the greatest altitude ; and in reference to which some error must obviously have crept in. The following Table shows Mr. Mitchell's results uniformly with those in the last Table. The fuses which he employed were fifteen-seconds or three-inch ones, and their times of combustion have therefore been multiplied by two in order to bring them into comparison with the six-inch fuses which were used in the author's experiments :—

Pressure of air in inches of mercury.	Average time of combustion of six-inch fuse.	Increase of time of com- bustion over last observa- tion.	Reduction of pressure cor- responding to increase of time.	Increase of time for each diminution of one-inch pressure.
	Seconds.	Seconds.	Inches.	Seconds.
29·61	28·50			
26·75	31·56	3·06	2·86	1·070
23·95	34·20	2·64	2·80	·943
22·98	36·25	2·05	·97	2·113

Here, omitting the last determination as abnormal, we have the average retardation, in the combustion of a six-inch fuse, for each diminution of one-inch mercurial pressure, equal to 1·007 second, which coincides almost exactly with the number (1·043) deduced from the author's experiments.

The results of both series of observations may therefore be embodied in the following law :—*The increments in time are proportional to the decrements in pressure.* For all practical purposes the following rule may be adopted :—*Each diminution of one inch of barometrical pressure causes a retardation of one second in a thirty-seconds fuse ; or, each diminution of atmospheric pressure to the extent of one mercurial inch increases the time of burning by one-thirtieth.*

This retardation in the burning of time-fuses by the reduction of atmospheric pressure will probably merit the attention of artillery officers. Up to the present moment these fuses have been carefully prepared so as to burn, at Woolwich, a certain number of seconds ; but such time of combustion at the sea-level is no longer maintained when the fuses are used in more elevated localities. Even the ordinary fluctuations of the barometer in our latitude must render the time of the combustion of these fuses liable to a variation of about ten per cent. Thus a fuse driven to burn thirty seconds when the barometer stands at 31 inches, would burn thirty-three seconds if the barometer fell to 28 inches. Even the height to which a shell

attains in its flight must exert an appreciable influence upon the burning of its time-fuse ; to a still greater extent, however, must the time of combustion be affected by the position of the fuse during the flight of the shell. If it precede the shell, the time of burning must obviously be considerably shorter than if it follow in the comparatively vacuous space behind the shell.

The apparently opposite conclusions to which we are led as regards the influence of atmospheric pressure upon the *rate* of combustion, by the experiments upon candles on the one hand and upon time-fuses on the other, are by no means irreconcilable ; in fact, an examination into the conditions of combustion in the two cases¹ scarcely leaves room for the expectation of any other result. In the combustion of a candle, the author proves that, at all pressures, there is a sufficient supply of melted combustible matter kept up at the base of the exposed portion of the wick : the capillarity of the latter is not affected by pressure ; and as the temperature of the flame is also proved to remain practically constant, effecting the evaporation of the same amount of combustible matter under all pressures, it follows that the rate of consumption of a candle must be nearly or quite independent of the pressure of the surrounding medium. In the deflagration of time-fuses, the conditions are obviously very different. Here the combustible matter never comes into contact with atmospheric oxygen until it has been ejected from the fuse-case. Unlike the candle, the composition contains within itself the oxygen necessary for combustion, and a certain degree of heat only is necessary to bring about chemical combination. If this heat were applied simultaneously to every part of the fuse composition, the whole would burn almost instantaneously. Under ordinary circumstances, however, the fuse burns only at a disk perpendicular to its axis ; and the time occupied in its deflagration necessarily depends upon the rapidity with which each successive layer of composition is heated to the temperature at which chemical combination takes place. This heat, necessary to deflagration, is evidently derived from the products of the combustion of the immediately preceding layer of composition ; and the amount of heat thus communicated to the next unburnt layer must depend, in great measure, upon the number of particles of these heated products which come into contact with that layer. Now, as a large proportion of these products are gaseous, it follows that, if the pressure of the surrounding medium be reduced, the number of ignited gaseous particles in contact at any one moment with the still-unignited disk of composition will also be diminished. Hence the slower rate of deflagration in rarefied air.

With regard to the effect of atmospheric pressure upon the light of gas-flames, the author thus expresses the conclusion arrived at :—*Of 100 units of light emitted by a gas-flame burning in air at a pressure of 30 inches of mercury, 5.1 units are extinguished by each reduction of one mercurial inch of atmospheric pressure. Hence the decrease in illuminating power is directly proportional to the decrease in atmospheric pressure.*

This law is also proved to apply to gas, the illuminating power of which has been doubled by naphthalization ; and consequently it may be regarded as applying to all flames in which hydrocarbons are the source of light.

The investigation has also been extended to the effect of compressed atmospheres upon the light of combustion. Great difficulties were experienced in this branch of the inquiry, as gas could not be used, and recourse must therefore be had to other combustibles, which, as already pointed out, are liable to certain irregularities. Owing to these and other difficulties, satisfactory determinations could only be made between one and two atmospheres. In these determinations, the lamp which replaced the experimental gas-flame was supplied with amylic alcohol—a liquid which, whilst affording an appreciable amount of light under one-atmosphere pressure, was found to burn under two atmospheres without smoke, although at a somewhat higher pressure it began to evolve unconsumed carbon. The results obtained approximate closely to those indicated by the above law, deduced from the corresponding determinations in rarefied atmospheres, as will be seen from the following Table, in which, the mean of eleven observations is given under each experiment ; the column headed “calculated” containing the numbers deduced from the rate of variation of luminosity in rarefied air :—

Pressure.	Illuminating Power.	
	Observed.	Calculated.
1 Atmosphere	100	100
2 Atmospheres first.	263·7	253
2 Atmospheres second.	261·3	253

Further determinations, in which the illuminating power at three- and four-atmospheres pressure was compared, yielded results differing widely from this law, and indicating a much more rapid increase of light ; but as the liability to errors increases greatly at these higher pressures, little confidence is placed in the numbers. The lamp was fed with a mixture of five parts of vinic alcohol and one part of amylic alcohol ; it had no appreciable illuminating effect under ordinary atmospheric pressure :—

Pressure.	Illuminating Power.	
	Observed.	Calculated.
3 Atmospheres	406	406
4 Atmospheres	959	559

In tracing the cause of this variation of light under different atmospheric pressures, the author calls attention to the conditions upon which the light of ordinary flames depends. He shows that it is derived almost exclusively from the separation of carbon particles within the flame, and that it is increased by the augmentation of the amount of carbon thus precipitated, and by an increased temperature in the flame ; whilst it is diminished by the separation of less carbon and by a reduction of temperature. The temperature of flame is not materially altered by the rarefaction of the supporting medium ; and hence the loss of light cannot arise from a reduction of temperature. On the other hand, the separation of carbon particles is greatly augmented by increased pressure ; thus candles evolve much smoke when burnt under a pressure of two atmospheres ; whilst even a small alcohol flame, which burns with a pure blue light at ordinary pressures, becomes highly luminous in air four times compressed. Flames which smoke at ordinary pressures become smokeless in rarefied air, and undergo more complete combustion. Whilst, therefore, the light of flames is due to the separation of carbon particles, the latter owe their momentary existence to the absence of sufficient oxygen for their combustion ; consequently any influence which causes the more rapid interpenetration of the flame gases and exterior air, must reduce the amount of precipitated carbon, and consequently also the luminosity of the flame. Rarefaction exercises precisely such an influence by increasing, as it is well known to do, the mobility of the gaseous particles, and thus causing the access of a larger amount of oxygen to the region of the flame where precipitated carbon produces luminosity.

An analysis of the gases evolved from a candle flame, burning under a pressure of only eight mercurial inches, proved that there was perfect combustion even at this low pressure.

In conclusion, the influence of atmospheric pressure upon the phenomena of combustion may be thus summed up.

1. The rate of burning of candles and other similar combustibles, whose flames depend upon the volatilization and ignition of combustible matter in contact with atmospheric air, is not perceptibly affected by the pressure of the supporting medium.

2. The rate of burning of self-supporting combustibles, like time-fuses, depends upon the rapidity of fusion of the combustible composition, which rapidity of fusion is diminished by the more rapid removal of the heated gases from the surface of the composition. Hence the rate of burning of combustibles of this class depends upon the pressure of the medium in which they are consumed. In the case of time-fuses, the increments in the time of burning are proportional to the decrements in the pressure of the surrounding medium.

3. The luminosity of ordinary flames depends upon the pressure of the supporting medium ; and, between certain limits, the decrease in illuminating power is directly proportional to the decrease in atmospheric pressure.

4. The variation in the illuminating power of flame by alterations

in the pressure of the supporting medium depends chiefly, if not entirely, upon the ready access of atmospheric oxygen to, or its comparative exclusion from, the interior of the flame.

5. Down to a certain minimum limit, the more rarefied the atmosphere in which flame burns, the more complete is its combustion.

“On the Anatomy and Physiology of the Spongiadæ.” Part II. By J. S. Bowerbank, LL.D., F.R.S., F.L.S. &c.

“Further Observations upon the *Nebulæ*, with practical details relating to the construction of large Telescopes.” By the Earl of Rosse, F.R.S.

In this paper the processes which were found best to answer in casting specula of 6-feet aperture have been described at some length; and the precautions which were taken to prevent accidents during the progressive stages of manufacture.

Some information, which may perhaps be useful, has also been gathered from the memoranda kept on each occasion when the specula were ground and polished; and the results of a long experience in the different manipulations have been put together in a practical form, so as best to enable the civil engineer to undertake the construction of large telescopes as a matter of business.

A few hints have been given on figuring and repolishing large surfaces, which the astronomer may find useful should his services be required in distant countries.

An attempt has been made to define the extreme limits to which telescopic vision may be pushed in this country, in answer to the question whether instruments of greater dimensions might not be advantageously constructed.

The peculiarities in the mounting of the large telescope are slightly noticed, and their advantages and disadvantages explained, as they have been brought out by experience.

This is followed by a selection from the observations made during a period of six years, accompanied by drawings of the more remarkable objects.

The principal results seem to be a large addition to the list of *nebulæ* with curved or spiral branches, and many new double and multiple *nebulæ*. A variety of objects have been also pointed out upon which the labour of a careful scrutiny will probably be amply repaid, with a similar instrument, even in this climate. A still larger number have been marked off, which to save time may be passed by, unless some new views on Cosmogony should suggest sufficient motives for reobserving them. A record has been made, which, to some extent, will be available hereafter for comparison with the heavens, and a few cases of suspected change have been noticed, where the evidence, however, is by no means conclusive,

“Observations on the Posterior Lobes of the Cerebrum of the *Quadrumanæ*, with a Description of the Brain of a *Galago*.” By William H. Flower, Esq.

“On Liquid Transpiration in relation to Chemical Composition.”
By Thomas Graham, Esq., V.P.R.S, Master of the Mint.

In accordance with the analogy of the transpiration of gases, the passage of liquids under pressure through a capillary tube is spoken of as liquid transpiration. The subject owes the development which it has already attained chiefly to the investigations of M. Poiseuille. The precision of the mode of experimenting pursued by that physicist has been remarked on by every one who has engaged in the inquiry. The same method was accordingly adopted with little variation in the present investigation.

The isolated observation made by M. Poiseuille, that alcohol diluted to different degrees is most retarded in passing through a capillary tube at that degree of dilution where the greatest condensation of the mixed liquids occurs, was understood by the author as indicating that the definite hydrate of alcohol containing six equivalents of water (or three equivalents of water with the formula of alcohol taken as $C_2H_5O_2$), was the most retarded in transpiration. The rate of transpiration appeared here to depend upon chemical composition, and to afford an indication of it. A new physical property may thus become available, like the boiling-point and others, for fixing the chemical constitution of substances. The same feature was recognized in methylic alcohol, although the six-hydrate here is not remarkable for condensation of volume; and the inquiry was then extended to the hydrated acids, and to other substances. The results appear to establish the existence of a relation between transpirability and composition.

The time of passage of equal volumes of different liquids under the same pressure and at the same temperature, may be termed their transpiration times, and be referred to the time of water as unity. The transpiration of nitric acid, NHO_6 , with and without water, at $20^\circ C$. was as follows :—

Water added to 100 Nitric acid (NHO_6).	Transpiration time (water = 1).
0	0·9899
25·47	1·9885
28·56 (2 equivalents)	2·0258
30	2·0459
40	2·0833
42·85 (3 equivalents)	2·1034 (the maximum).
45	2·0977
50	2·0919
55	2·0632
57·12 (4 equivalents)	2·0459
60	2·0387
70	1·9626
80	1·8994
90	1·8261
100	1·7040
200	1·3563

The transpiration time rises with successive additions of water, till the proportion corresponding to three equivalents is reached, when the time is 2·1034, and has attained its maximum. Diluted beyond this point the nitric acid begins to pass more freely, and the transpiration time approaches again to that of water. The hydrate named, $\text{NHO}_3 + 3\text{HO}$, having sp. gr. 1·4, possesses the highest boiling-point, and the character of definite composition. It is what I have elsewhere spoken of as the “constitutional” hydrate of nitric acid.

In acetic acid, the constitutional hydrate, $\text{C}_4\text{H}_4\text{O}_4 + 2\text{HO}$, is indicated by transpiration with equal precision. The transpiration time rises from 1·2801, the time of the basic hydrate, $\text{C}_4\text{H}_4\text{O}_4$, to 2·7040, the time of the hydrate first referred to; falling again afterwards as the water is increased. Butyric and valerianic acids present the same character, although slightly modified. Formic acid, on the other hand, departs entirely from the acetic type in transpirability, as it does in the density of its combinations with water, and in its indisposition to form basic salts. It is curious that liquid formic acid, although an acetic acid by derivation, should more resemble hydrochloric acid in physical characters.

The transpiration time of sulphuric acid is 21·6514, a high number, as might be expected from the viscosity of the liquid. But the time rose still further with the addition of water, till 17·5 parts of water were added to 100 of oil of vitriol, when the number was 23·7706. The proportion of water stated approaches closely to 18·36 parts, which represent 1 equivalent. Here again a well-known constitutional hydrate is indicated, $\text{SHO}_4 + \text{HO}$.

In hydrochloric acid the only sensible retardation observed was with the hydrate represented by $\text{HCl} + 12\text{HO}$. This is the hydrate which possesses least volatility at the low temperature of the experiment (20°C).

It was supposed that glycerine, as a triatomic alcohol, might affect combination with water in the proportion $\text{C}_6\text{H}_8\text{O}_6 + 18\text{HO}$. But no such compound was indicated by transpiration of the aqueous solutions of glycerine.

The transpiration of pure acetone is remarkably rapid, and is greatly retarded by the addition of water. The time rises from 0·401, that of anhydrous acetone, to 1·604, the time of the twelve-hydrate, taking the equivalent of acetone as $\text{C}_6\text{H}_6\text{O}_2$, or of the six-hydrate with the equivalent $\text{C}_3\text{H}_3\text{O}$.

The transpiration times and boiling-points of three alcohols are as follows :—

	Transpiration time.	Boiling-pont.
Methylic alcohol	0·630	66 C.
Vinic alcohol	1·195	78·5
Amylic alcohol	3·649	132

Of four others:—

	Transpiration time.	Boiling-point.
Formic ether	0·511	55·5
Acetic ether	0·553	74·0
Butyric ether	0·750	
Valerianic ether	0·827	133·5

Judging from these last observations, the order of succession of individual substances in any natural series will be indicated by the individual transpirability of these substances, as clearly as it is by their comparative volatility. Transpiration and boiling-point observations may come thus to claim an equal interest. In carrying out the inquiry, it will probably be found advantageous to transpire the liquids at a fixed temperature which is somewhat elevated. A large number of substances are liquid at 100° C., of which the transpiration times could easily be obtained. Slow transpiration and low volatility appear to go together, and both to be connected in a general way with a heavy molecule. So also the annexation of constitutional water to the hydrated acids and alcohols appears to impede the transpiration of these substances.

GEOLOGICAL SOCIETY.

[Continued from p. 166.]

June 4, 1862.—Prof. A. C. Ramsay, President, in the Chair.

The following communications were read:—

1. “On the disputed affinities of the Purbeck Mammalian genus *Plagiaulax*.” By Hugh Falconer, M.D., F.R.S., F.G.S.

The author first referred to his former description (in the Journal of the Society, vol. xiii. p. 261, &c.) of *Plagiaulax* as a rodent form of Marsupial, having affinities with the existing *Hypsiprymnus*; and then he drew attention to the very different opinion of its relationship expressed by Professor Owen in the article “Palæontology” in the 8th edition of the ‘Encyclopædia Britannica,’ and in the reprint of that article as a separate work, where it is said to have been “a Carnivorous Marsupial.”

Dr. Falconer then reviewed the dental characters of *Plagiaulax* in detail. Of the incisors he said that, in regard of number, order of suppression, collateral position, and relation to the premolars, they correspond exactly with the type of Marsupial herbivora, and are wholly at variance with the carnivorous type; and he argued that Professor Owen’s argument drawn from *Thylacoleo* has no bearing on the incisors of *Plagiaulax*, and gives no support to the carnivorous inference. Of the premolars, after a full comparative re-examination, Dr. Falconer stated that, in agreement with his already published views, he finds reason to regard the carnivorous deduction from the shape of the premolars to be untenable.

The form of the lower jaw of *Plagiaulax* having been regarded by Professor Owen as conforming with the carnivorous type, the author

showed that in the non-carnivorous *Cheiromys* (Aye-Aye) and *Phascolarctus* (Koala) a similar form of ramus obtains; and that the coronoid and condyle of the Aye-Aye are not unlike those of *Plagiaulax*, whilst in existing predaceous Marsupials the condyle has a different form. The author is altogether confirmed in his opinion that *Plagiaulax* was essentially a phytophagous marsupial.

2. "On some Fossil Plants from the Hempstead Beds, Isle of Wight." By the Rev. Dr. O. Heer, Professor of Botany, Zurich. With an Introduction, by W. Pengelly, Esq., F.G.S.

These plant-remains, collected by Mr. H. Keeping, under Mr. Pengelly's direction, from the Hempstead Series, consist of seeds, cones, leaves, and twigs, and are referable to ten species, four of which have been found lately at Bovey Tracey also, namely, *Sequoia Couttsiæ*, Heer, *Andromeda reticulata*, Ettin., *Nymphæa Doris*, Heer, and *Carpolites Websteri*, Brongn. The other species are *Cyperites Forbesi*, sp. nov., *Nelumbium Buchii*, Ettin., *Carpolites globulus*, sp. n., *Chara Escheri*, Braun, and *Ch. tuberculata*, Lyell, var. Professor Heer notices that six of the above-named species are found also in the Lower Miocene (Tongrian and Aquitanian) of the Continent, and that this flora, as far as represented, seems to indicate truly freshwater conditions for the formation in which it is found.

3. "On Glacial Surface-markings on the Sandstone near Liverpool." By G. H. Morton, Esq., F.G.S.

The author here noticed the occurrence of glacial grooves and scratches—1st, at Toxteth Park, the direction of the striæ being N. 42° W., at 120 feet above the sea; 2nd and 3rd, at Boundary-lane and New-road, Kirkdale, the striæ being N. 15° W., and at about 80 feet above the sea.

XXXIII. Intelligence and Miscellaneous Articles.

ON GROUND-ICE, OR ANCHOR ICE, IN RIVERS.

BY PROFESSOR JAMES THOMSON.

IN this paper the author described the two principal modes of growth of ice, in still water and in running water. In still or slowly-moving water the ice forms itself as a crust on the surface, because, as the water cools from about 40° F. down to the freezing-point, it expands, and therefore becomes lighter, and remains floating at the surface, and then, on freezing there, it expands still further, and therefore still more tends to float. In rapidly-moving river water, on the contrary, and especially at the foot of rapids, ice is often found to grow attaching itself to the rocks or stones forming the bed of the river, as a spongy or porous mass, which, seen in the aggregate and not examined minutely, presents a general appearance not unlike the spawn of frogs. In large rivers in cold climates, as, for instance, in the St. Lawrence, immense quantities of this ice, called ground- or

anchor ice, are found to accumulate with astonishing rapidity. These accumulations of ice, by damming up the water, cause great floods, and by yielding to the force of the water, and moving down with the current, especially after they have become jammed and heaped up with other ice formed on the surface, act in producing very striking geological effects in disturbing the bottom and banks of the river, and in shoving along huge boulders which otherwise would remain immovable. The ground- and surface-ice, also, by their shoving-action, introduce formidable difficulties and dangers in the construction of bridges or other engineering works requiring to be founded on the beds of rivers in cold climates. In the construction of the Great Victoria Bridge across the St. Lawrence at Montreal (the most costly bridge which has ever been executed), these difficulties have been successfully overcome, and a structure has been raised which is likely to stand secure against the much-dreaded forces of the ice. On account of the tendency both of water approaching to the freezing-point and of ice to float, it has long been regarded as rather a singular circumstance that ice should ever be found growing at the bottom of a river. From among the many suggestions which have been offered at various times to account more or less completely for the phenomenon, the author sets out by accepting as quite correct the view that the essential difference between the circumstances of the freezing of lake and river water is, that in the former case the water is left undisturbed to the action of the cold, and is allowed to adjust itself in strata in which the coldest parts, being also the lightest, float to the top; while in rivers the whole water is, by mixing, due to its rapid flow, brought to a uniform temperature at the freezing-point from top to bottom, and is thus brought into a condition in which it is ready to freeze at any part where additional cold may be applied. He is not, however, satisfied with any of the numerous suggestions which have been offered to account for the growth of the masses of spongy ice at the bottom, rather than that the ice should be found at the top, or in a state of mixture with the water throughout its depth. Some, for instance, have thought that radiation from the bottom to a cold sky (see paper by the Rev. James Farquharson, *Philosophical Transactions*, 1835) would cause ice to grow at the bottom of the river much in the same way as hoar-frost grows on land. Arago, having rejected the supposition of radiation being the cause, assigned two other reasons: first, that there might be expected to be a peculiar aptitude to the formation of crystals on the stones and asperities at the bottom, like as there is found to be a special readiness for the formation of crystals on rough bodies in saline solutions; and secondly, he supposed that the existence of less motion of the water at the bottom would favour the growth of the crystals there. As against this view, the author of the present paper states, first, that the water of a rapid river when freezing has abundance of small spicula or fragments of ice floating diffused through it, every one of which offers at least as free a point for the reception of new ice crystallizing from the water as can be presented by asperities on the bottom; and secondly, that the slower motion at the bottom

would not favour the occurrence of freezing of new ice there rather than at the top, but that, on the contrary, if effects on the tendency to crystallization are to be sought for in such a slight cause, it should rather be taken that the greater fluid friction at the bottom, and the heavier pressure there, are causes slightly, *but certainly very slightly*, tending to oppose the freezing of new ice at the bottom.

Mr. Hodges, the engineer of the contractors for the great bridge across the St. Lawrence at Montreal, in his large and valuable work recently published (in 1860) on the construction of that bridge, describes the ice-phenomena of the St. Lawrence, which he had been obliged during many years to watch and inquire into with anxious care; and in respect to the origin of the ground-ice, he supposes that the water in passing down rapids may become aërated by the rapidity of the current, and that particles or globules of cold air, being whirled by the eddies till they come in contact with the rocky bed of the river, attach themselves to it, and there give out cold which they have brought with them from the very cold atmosphere above, and so induce the freezing of ice around themselves in adhesion to the bottom of the river. As against this speculation, the author of the present paper states that the cold which could be conveyed down into the water by small bubbles would be totally inadequate to produce the results in question, and that any freezing which small bubbles of air could produce would occur during the period of their eddying about through the water, rather than at a later time, when their temperature would be assimilated to that of the water. The author's view, which it was the chief object of the paper to present, is that crystals or small pieces of ice are frozen from the water at any part of the depth of the stream, whether the top, the middle, or the bottom, where cold may be introduced either by contact or radiation, and that they may be supplied in part by snow or otherwise; and that they are whirled about in currents and eddies until they come in contact with any fixed objects to which they can adhere, and which may perhaps be rocks or stones, or may be pieces of ice accidentally caught in crevices of the rocks or stones, or may be ground-ice already grown from such a beginning. The growth of the ice by adhesion of new particles formed elsewhere he attributes to the property of any two pieces of moist ice to adhere when brought into contact, which has been a subject of much discussion of late years, and of which the author's views are to be found in various recent papers in the 'Proceedings of the Royal Society,' and have also been submitted from time to time to the Belfast Natural History and Philosophical Society. He is confident that the anchor ice is not formed by crystallization at the place where it is found adhering. He is aware that the idea has sometimes been mooted, that snow falling into rivers might somehow be converted into anchor ice; but he is not aware that hitherto any explanation has been offered coupling the formation of the anchor ice with the property of ice now commonly designated as "regelation," but which until late years was not very generally known or understood, more especially as a property capable of bringing about the union of small pieces of ice floating freely under

water*: and the mode of growth of ground-ice is, he believes, as yet commonly regarded as an unsettled point, no opinion offered having received very decisive or general assent.—*Proceedings of the Belfast Natural History and Philosophical Society*, May 7, 1862.

ON THE TEMPERATURE OF THE INTERIOR LAYERS OF THE AIR.

BY M. BECQUEREL.

M. Becquerel has proposed in his Memoir to present a *résumé* of the observations made by him at the Jardin des Plantes on the temperature of the air, in which he more especially tries to show that this temperature in the lower layers depends, as is known, not only on the terrestrial radiation and on the celestial radiation, but also on the direct radiation of the sun. In commencing, he has pointed out the influence of the soils, according to their nature and physical condition, in raising or lowering the temperature, up to a certain height, when they are heated by solar radiation, or cooled by nocturnal radiation.

If, with Schubler, the faculty which calcareous sand possesses of retaining heat be called 100, a faculty which depends on its radiating, absorbing, and conducting powers, we have—

For siliceous sand	95·6
For arable calcareous soil	74·5
For argillaceous soil	68·4
For garden earth	64·8
For humus	49·0

As humus has only half the power of calcareous sand, it cools in less time than the latter. The size of the parts ought to be taken into account: other things being equal, the siliceous and calcareous sands, as compared with equal volumes of different argillaceous or calcareous earths in fine powder, of humus, of arable land, and of garden earth, are the soils which appear to conduct heat least. Hence during night in summer, sandy soils retain a higher temperature than other earths.

A soil covered with siliceous pebbles cools even more slowly than siliceous sands; hence for viniculture it is better than cretaceous or argillaceous soils, in which the maturity of the grape is more slowly effected.

As these soils, once heated by the sun's rays, do not cool in the same time, they do not act equally by radiation on the surrounding air, so that at a given time the temperature of the air is not the same for the same height above each of them; it remains for a longer time higher in a pebbly soil than in a calcareous or argillaceous one.

Hence in the same latitude, in the same conditions as regards protection, in places not very distant, and the soil of which is not the same,

* See Professor Faraday's paper in *Phil. Mag.* S. 4. vol. xxi. p. 146; and Professor Thomson's paper in the same *Journal*, vol. xxiii. p. 407.

the mean temperature is different. We see from this the difficulties in the exact determination of the temperature of a place, which is one of the elements taken into account in classifying climates.

In his memoir, M. Becquerel gives all the mean temperatures at 6 and 9 o'clock in the morning, 3 and 9 in the evening, at 1·33 metre at the north, and at midday, at 16 metres and at 21 metres above the soil, from the 1st of June 1860 to the 1st of June 1862, and the differences between these temperatures, as well as the mean temperature and those of the seasons. The discussion of the results obtained by comparing the observations made in this memoir leads to the following consequences.

The solar radiation increases the temperature of the air exposed to its action, an effect which does not take place in the north, where the instruments are protected from this action. This effect is independent of the terrestrial action when the soil has been heated.

The increase of temperature with the height, up to 21 metres, is put in evidence in the Tables of observation.

It is difficult to admit that the temperature of the air observed at the north, as usually done, exactly represents the temperature of the air resulting from the mixture of layers of air not having the same temperature.

The terrestrial radiation, according as the soil has been heated by the solar action or cooled by the celestial radiation, doubtless exercises a great influence on the temperature of the air, to a height which depends on the nature of the soil and of that of the bodies which cover it, an influence which has hitherto been too much neglected in observations on the temperature of the air; but it is necessary to take into account the direct action of the sun on the air, which possesses, in fact, a feeble absorbing power that increases with its density in approaching the soil.—*Comptes Rendus*, June 22, 1862.

RESEARCHES ON THE REFRACTIVE INDICES OF BODIES WHICH ONLY ASSUME THE GASEOUS CONDITION AT HIGH TEMPERATURES. ABNORMAL DISPERSION OF IODINE VAPOUR. BY M. F.-P. LEROUX.

The kindness with which the Academy has received my first attempts* to study the refraction produced by gaseous media, has encouraged me to continue these researches in spite of the numerous experimental difficulties which they present. I have proposed not only to extend them to a larger number of substances, but also to study the dispersion produced by gaseous media.

In the present communication I draw the attention of the Academy to the dispersion produced by iodine vapour. This body deserves, in fact, special mention, both from the difficulties of observation of all kinds which it offers, and from the novelty of the phenomenon to which it gives rise.

* *Phil. Mag.* vol. xxi. p. 296.

Iodine vapour disperses light in a different direction to any substance yet studied; that is, a prism full of iodine vapour refracts red rays to a greater extent than blue rays.

In the first communication I made on this subject, I said that, by filling with iodine vapour the prism of my apparatus, "the image of a brilliantly illuminated slit appeared composed of two distinct juxtaposed parts, one red, the other blue." I had remarked that the order of the two colours was not the same as in the spectra produced by all the substances previously studied. Yet I would not announce this result, so contrary to what is usually seen, until I had studied it in all its details, and especially after having arranged my apparatus so as to enable other persons to witness the phenomenon.

After several attempts, I had first to get prisms of porcelain made; for metal prisms, whether gilt or enamelled, did not resist the action of iodine vapour. I had then to combine my system of illumination, so as to overcome by a sufficient luminous intensity the opacity of iodine vapour. To put my experiments beyond a doubt, I had then to study the appearances observed when a luminous image is viewed by a small number of simple lights. I had thus occasion to put in evidence the want of achromatism of the eye, which led to a great number of consequences. This forms the subject of a research which I communicated to the Academy about six weeks ago, and which now appears in the *Annales de Chimie et de Physique*.

As confirmation of the fact which I advance, the following are the various proofs to which I have submitted it:—

1. The effect observed does not depend on a special or accidental arrangement of the apparatus, or of the glasses which compose the prism, since different prisms formed by different glasses always give the same result.

2. A glass prism which gives a deviation in the same direction as the prism of iodine vapour, and almost equal to it (about $11'$), virtually achromatizes the image; the dispersion of iodine vapour is thus opposite that of glass.

3. In successively illuminating the slit of my collimator by the red and bluish violet arising from the dispersion of a solar beam by a flint-glass prism, the red and the blue image are produced at different parts. An experiment of the same kind may be made by interposing different coloured-glasses. This shows that the refrangibility of the red ray is really greater than that of the blue ray in the iodine vapour; hence the phenomenon cannot be explained by a transformation of radiations, since the light which is red on entering the prism is red on emergence, and so on.

4. The prism being placed in air, the effect is complicated by the dispersion produced by this medium; but a direct experiment showed me that the dispersion produced by the air under the same circumstances was only a few seconds, while that of iodine is about $30''$.

The dispersive power of iodine varies inversely as the temperature.

Besides red and blue, iodine allows ultra-violet rays to pass; hence the precision of the phenomenon is increased by purifying, by means

of a uranium glass, the solar light which illuminates the slit of the collimator.

Such are the phenomena which for the last two years I have studied whenever the sun allows. I am now in a position to enable members of the Academy interested in the phenomenon to witness it.—*Comptes Rendus*, July 21, 1862.

ON THE CALM LINES OFTEN SEEN ON A RIPPLED SEA.

BY PROFESSOR JAMES THOMSON.

In this paper the object of the author was to offer a new explanation of the origin of lines of glassy-calm water, usually long and sinuous, which are often to be seen extending over the surface of a sea darkened elsewhere by a ripple. He adverted to the commonly received supposition that these lines are due to some kind of oily film on the surface of the water, and to the prevalent idea that the oil is somehow given off from shoals of fishes. These suppositions, he thought, although having some slight foundation in the facts of the case, did not form the true explanation of the phenomenon. His brother, Professor William Thomson, had observed, and had pointed out to him, that the water at the calm lines always contains considerable quantities of small floating objects, such as little detached pieces of seaweed, leaves of trees, or the like, and had accounted for the smoothness of the water by the friction induced among the little undulations by the presence of those solid objects. The question still remained, however, as to what might be the cause of the leaves and seaweed being arranged in such long and sinuous lines. One of these calm lines was noticed last autumn in Brodick Bay by Professor William Thomson and the author; and on rowing into it, they found leaves and seaweed abundantly diffused through the water there, while the rippled water on both sides of it was comparatively free from such objects. The line of calm water evidently sprang from a point on the shore where a small river entered the sea, and there could be no doubt that the leaves were supplied by that river. Still it appeared an untenable supposition that the river water could extend so very far out to sea, and wind about over the surface of the sea as the calm line did, sometimes even narrowing instead of spreading out as a broad sheet, which the light fresh water flowing over the heavy salt water should be expected to do. It occurred to the author that the water of the river would actually spread out as a broad sheet over the surface of the sea; and that in its outward lateral flow it would constantly be carrying with it the leaves with which it was originally charged, and all such small pieces of seaweed as it might meet with in the sea-water, and that these would accumulate in a boundary line of the region of dispersion, which might be determined by some slight flow of the surface of the adjacent sea-water meeting this outspreading fresher water, and causing a downward or sinking motion, however slight, of the two

meeting currents. The author does not mean to attribute the calm lines in general to the spreading of a sheet of fresh water over the salt water of the sea, but he thinks the general explanation is readily developed from the observations and explanation in the foregoing particular case. He supposes in general that in estuaries and channels of the sea, and in lakes and rivers, the water must often be affected by various causes, such as tides, breezes, currents, and circulation due to differences of heat, so as to be made to rise occasionally at some places and to sink at others. Now along the line of meeting and sinking of two opposing surface currents, all floating objects carried by those currents will be collected together; and there they will act as dampers, or floating breakwaters, for the small ripple undulations. The slightest possible inequalities of the forces of the two opposing currents will suffice to account for the great sinuosities which the calm lines show. If there should happen to be any oily scum thinly diffused on the surface of the water, this will be brought together, along with seaweeds, &c., from a wide area, to the line of meeting and descent of the two opposing surface currents; and to an oily film having been frequently noticed on the calm places, though really brought to them in the way now suggested, the author inclines to attribute the supposition that it is their cause; but he agrees that very possibly, too, an oily scum thus sometimes brought together may cooperate with the seaweed and other floating objects in resisting the propagation of the ripple.

In the discussion which followed the reading of this paper, it was remarked by members of the Society that, in drawing nets through the sea near its surface for collecting small marine animals, they had often found that these creatures were present in vastly greater abundance in the calm lines than in the rippled sea; and it was suggested that, as the bodies of many of these contain oil, this, on their dying, might be given off from their bodies, and might float on the surface, and might cause the smoothness there observed; and it was also stated that shoals of herrings are much more abundantly met with under the calm lines than at other parts of the sea.

The author of the paper, in replying, pointed out that, if marine animals giving off oil from their bodies were really the cause of the calm lines, it would still remain as an unsolved question, and one which would then be of great interest, Why should those animals be found to congregate in long sinuous lines, extending often continuously for miles over the surface of the sea? He thought, however, that the true state of the case would probably be, that the small animals are brought together by the same currents which, according to his supposition, collect the seaweed, leaves, and other floating objects into lines, and that the animals, not wishing to descend with the meeting currents into deep water, remain near the surface, and that the herrings or other fishes congregate to the same lines in order to feed on the smaller animals.—*Proceedings of the Belfast Natural History and Philosophical Society*, May 7, 1862.

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[FOURTH SERIES.]

OCTOBER 1862.

XXXIV. *On some Properties of the Induced Current.* By Dr. P. L. RIJKE, *Professor of Natural Philosophy in the University of Leyden**.

1. **T**HE mode in which an inducing current is broken, exercises, as is known, a considerable influence on the striking distance of the induced current. Although in most cases we could not account for every increase or decrease in length which the induction spark undergoes when we use a given mode of breaking contact, yet some results have appeared capable of being reduced to a general principle; and thus the experiments of MM. Fizeau and Poggendorff have shown that everything which tends to diminish the electric density at the place where the circuit is broken, exerts a favourable influence on the length of the spark of the induced current.

In referring to the principle to which we have just alluded a fact known for some time (the instantaneous loss of electricity which every conductor undergoes when placed in contact with a flame), I have been led to think that it would be possible to increase the striking distance of an induced current, by effecting the interruption of the inducing current in the middle of a flame.

2. The experiments which I am about to describe have confirmed these anticipations.

The rupture always took place between platinum wires terminated in the form of a ring, which were made to slide rapidly one along the other. Of course these wires, which were held in the hand, were insulated sufficiently to prevent the extra current from passing through the arms of the operator. The ends of the induced wire were connected with a spark-micrometer. The spark passed between two copper knobs, each 16·6 millims. in

* Communicated by the Author.

diameter. When the flame was produced by the combustion of a gas, a Bunsen's lamp was used. In these experiments the lower orifices of the lamp were closed, so that there was not a mixture of air and gas. I have always used one of the large-sized Ruhmkorff's coils. Two Bunsen's elements were ordinarily used, each consisting of plates which exposed 352 square centimetres of surface.

The following are the first results which I obtained:—

Medium in which the inducing circuit was broken.	Greatest striking distance.	Ratio between the striking distances obtained when the circuit is broken in the given medium and in air.
Atmospheric air ...	<div style="text-align: right;">millims.</div> <div style="text-align: center;">{ 8.1 }</div> <div style="text-align: center;">{ 8.1 }</div> <hr style="width: 50px; margin: 0 auto;"/> <div style="text-align: right;">Mean..... 8.1</div>	1
Alcohol flame	<div style="text-align: center;">{ 19.3 }</div> <div style="text-align: center;">{ 19.5 }</div> <hr style="width: 50px; margin: 0 auto;"/> <div style="text-align: right;">Mean..... 19.4</div>	2.395
Hydrogen flame ...	<div style="text-align: center;">{ 56.6 }</div> <div style="text-align: center;">{ 58.6 }</div> <hr style="width: 50px; margin: 0 auto;"/> <div style="text-align: right;">Mean..... 57.6</div>	7.110
Coal-gas flame.....	<div style="text-align: center;">{ 57.2 }</div> <div style="text-align: center;">{ 59.0 }</div> <hr style="width: 50px; margin: 0 auto;"/> <div style="text-align: right;">Mean..... 58.1</div>	7.173

Thus the action of an alcohol flame was far inferior to those which hydrogen and coal-gas flames exert. The last two appear to produce almost the same effect.

3. I am stating nothing new to most persons in mentioning that the shape of the electrodes between which the discharge of a conductor of a Leyden jar, or of an induced current, is made to pass, exercises a considerable influence on the striking distance; and every one knows, further, that this distance increases considerably when a tapering conductor is taken for the positive electrode, and for the negative electrode a plate arranged perpendicularly to the first electrode. It remained to inquire if induction sparks, which, by being made to strike between new electrodes, were able to cross greater distances, could be further elongated when the rupture of the inducing circuit, instead of taking place in air, took place in a flame. The following results prove that this question ought to be answered affirmatively, excepting merely that the increase is not so considerable:—

Medium in which the rupture of the inducing circuit took place.	Greatest striking distance.	Ratio between the striking distances obtained when the rupture takes place in the given medium and in air.
Atmospheric air ...	<div> <div>millims.</div> <div>{ 20·4 }</div> <div>{ 17·4 }</div> <div>Mean..... 18·9</div> </div>	1
Alcohol flame	<div> <div>{ 31·4 }</div> <div>{ 30·7 }</div> <div>Mean..... 31·05</div> </div>	1·643
Hydrogen flame ...	<div> <div>{ 76·9 }</div> <div>{ 77·3 }</div> <div>Mean..... 77·05</div> </div>	4·077
Coal-gas flame ...	<div> <div>{ 73·0 }</div> <div>{ 76·9 }</div> <div>Mean..... 74·95</div> </div>	3·966

The copper plate was 35·4 millims. in diameter. The tapering conductor, also of copper, was 9 millims. thick at its base, and was 40 millims. in length.

4. It appeared to me not unimportant to examine if the influence of the flame continued to show itself when the two ends of the inducing wire were connected with the armatures of M. Fizeau's condenser. The following are the results obtained by interposing the condenser with which the apparatus is provided:—

Medium in which the rupture of the inducing circuit took place.	Electrodes used.	Greatest striking distance.	Ratio between the striking distances obtained when the rupture took place in the given medium and in air.
Atmospheric air ...	Knobs	<div> <div>millims.</div> <div>95·2</div> </div>	1
Coal-gas flame ...	Ditto	113·2	1·88
Atmospheric air ...	Tapering cylinder and plate	118	1
Coal-gas flame ...	Ditto	165	1·4

The examination of the results given in the last two Tables proves that *the influence of the flame is less perceptible when the experiment has been so arranged as to obtain a greater striking distance.*

5. The platinum wires may be left in the flame for a very short space of time—that necessary to produce the rupture; or, on

the contrary, before separating them they may be left to stay for a longer time, so as to allow them to assume a higher temperature. I have not noticed that the striking-distance in the one is greater than in the other case. When the platinum remains for a long time in the flame, its surface becomes roughened, it does not slide so easily, the duration of the rupture increases, and hence the striking-distance diminishes.

6. If platinum wires are taken so thin that they assume a very high temperature in the flame, and if they are withdrawn from the flame while still in contact, and then contact is rapidly broken without allowing them time to cool perceptibly, an explosive distance is always obtained equal to that which would have been found if contact had been broken in air at the ordinary temperature. Hence it follows that the increase in the striking-distance is simply due to the presence of a body in combustion, and not to the higher temperature of the electrodes.

I thought it necessary also to study the action exerted by the different parts of the flame. The results obtained are given in the following Table. The flame was produced by the combustion of coal-gas. The electrodes of the induced current were a pointed conductor and a circular plate:—

Medium in which the rupture of the inducing circuit took place.	Greatest striking distance.	Ratio between the striking distances obtained when the rupture takes place in the given medium and in air.
Atmospheric air	millims. $\begin{Bmatrix} 21.7 \\ 22.1 \end{Bmatrix}$ <hr/> Mean..... 21.9	1
Hottest part of the flame ...	$\begin{Bmatrix} 23.3 \\ 23.7 \end{Bmatrix}$ <hr/> Mean..... 23.5	1.073
Luminous part of the flame.	$\begin{Bmatrix} 59.2 \\ 49.7 \end{Bmatrix}$ <hr/> Mean..... 54.45	2.486
Lower part of the flame.....	$\begin{Bmatrix} 76.1 \\ 65.6 \end{Bmatrix}$ <hr/> Mean..... 70.85	3.235

Hence it is not the hottest part of the flame which exerts the greatest influence; on the contrary, it is that where the temperature is lowest.

7. Dr. Levoir, who assisted me in these experiments, surprised, like myself, at the result at which we had arrived, and which we were far from expecting, proposed to me to try whether, if the circuit were broken in a current of gas *not lighted*, we should not also find the striking distance increased. We immediately made the experiment, and found, in fact, that *gas not lighted produced an action but little less than that of flame*, as will be seen from the following Table:—

Medium in which the rupture of the inducing circuit took place.	Greatest striking distance.
Coal-gas not lighted	<div>millims.</div> <div>{ 63·2</div> <div> 63·6</div> <hr/> <div>Mean..... 63·4</div>
Lower part of the flame.....	<div>{ 64·1</div> <div> 67·3</div> <hr/> <div>Mean..... 65·7</div>

The electrodes between which the induction spark passed were the circular plate and the tapering conductor described above.

We found analogous results in working with hydrogen.

Medium in which the rupture of the inducing circuit took place.	Greatest striking distance.
Hydrogen lighted	<div>millims.</div> <div>{ 40·1</div> <div> 40·3</div> <hr/> <div>Mean..... 40·2</div>
Hydrogen not lighted	<div>{ 29</div> <div> 33</div> <hr/> <div>Mean..... 31</div>

The following are a series of experiments made with other gases* :—

* It is very rare that a spark is produced in a current of hydrogen without an explosion ensuing. The diffusion of air into hydrogen takes place so rapidly, that an explosive mixture is soon formed. This difficulty may be easily overcome by arranging the experiment in the following manner :— A glass funnel, so large that the two hands can be introduced, is fixed by means of a support in a vertical position, with the tube upwards. When a somewhat rapid current of hydrogen is passed into this tube, the circuit may be broken in the funnel without danger of an explosion. Moreover, an explosion under these circumstances would not present the same danger.

Medium in which the rupture of the inducing circuit was made.	Greatest striking distance.
	millims.
{ Atmospheric air	20
{ Carbonic acid	22
{ Ditto	21
{ Ditto	21
{ Atmospheric air	21
{ Atmospheric air	19
{ Ammoniacal gas	31
{ Ditto	27.5
{ Atmospheric air	18

The ratio between the striking distances obtained by breaking the circuit in various gaseous media is, for

Carbonic acid and air 1.01
 Ammonia and air 1.58

I do not dare to affirm that the slight increase produced by carbonic acid may not be attributed to an error of observation; but the considerable increase obtained by working with ammonia must doubtless arise from a specific action of this gas.

The next day, working with oxygen, nitrogen, and chlorine, I found the following results:—

Medium in which the rupture of the inducing circuit took place.	Greatest striking distance.
	millims.
{ Atmospheric air	17.1
{ Oxygen	19.5
{ Atmospheric air	18.5
{ Atmospheric air	16.8
{ Nitrogen	17.5
{ Ditto	16.0
{ Ditto	22.5
{ Ditto	18.0
{ Atmospheric air	16.2
{ Chlorine	15.0
{ Ditto	12.5
{ Ditto	14.5
{ Ditto	9.5
{ Atmospheric air	13.5

The ratio between the striking distances obtained by breaking the circuit in these different media was, for

Oxygen and air 1.096
 Nitrogen and air 1.075
 Chlorine and air 0.872

The numbers differ so little from unity, that I do not venture to say that the differences ought not to be attributed to accidental causes.

8. We found that when the circuit was broken in nitrogen, the greatest striking distances were the following :—

17·5
16
22·5

The gas issued from the gas-holder under various pressures. The greatest pressure having coincided with the greatest striking distance, I was led to inquire if this increase in the length of the spark arose simply from an accidental cause, or if it must be admitted that a certain degree of velocity imparted to the gaseous molecules exerted a favourable influence on the length of the spark. I undertook some experiments to settle this question. At first I found that sparks were obtained which had virtually the same length whether the rupture took place in air at rest, or in a current escaping from a Pepys's gas-holder. The case was different when the rupture took place in a *soufflet d'enfer*. By raising the air in this apparatus to its greatest pressure, in other words, giving to its molecules the greatest velocity, I obtained sparks 41 millims. in length, while the length was only 20 millims. when the rupture took place in air at rest.

In the reservoir of a Natterer's apparatus I condensed air under a pressure of 25 atmospheres. Effecting the rupture of the circuit in one case in the current of air which issued from the efflux-tube used for collecting carbonic acid, and in the other in air at rest, I obtained the results given in the following Table. Two copper knobs formed the electrodes between which the induction spark passed :—

Medium in which the rupture of the inducing circuit took place.	Greatest striking distance.	Ratio of the striking distance in the given medium to that in air.
	millims.	millims.
Current of air.....	69·6	8·09
Air at rest	8·6	1
Current of air.....	78·6	11·07
Air at rest	7·1	1

Hence we see that *the length of the induction spark increases considerably when the rupture of the inducing circuit takes place in air in rapid motion.*

9. It is known that a greater striking distance is obtained when the rupture of the inducing circuit is effected in water instead of in air. I found that *this distance is still more increased when a certain velocity is imparted to the liquid particles.* The following are the results obtained :—

Medium in which the rupture of the inducing circuit took place.	Greatest striking distance.
	millims.
Atmospheric air	8·6
Water at rest	33·6
Jet of water	57·6

I worked with ordinary rain-water, the jet was produced by a pressure of about 10·5 millims. The electrodes were the same knobs of copper as those used in the preceding experiment.

10. It has been sufficiently established by many physicists, that when an induced current is passed through the wire of a multiplier, the deflection obtained is quite independent of the mode in which the interruption is effected. It is simply necessary that the rupture takes place in a space of time very short compared with an oscillation of the magnetic needle. It has been concluded that the quantity of electricity which the interruption of the primary current sets in motion in the induced wire remains the same, whatever be the manner in which the rupture is made, for example in the middle of a flame. Does not this increase of the striking distance appear to prove that, when the rupture takes place under these conditions, what is called the electromotive force of the induced wire experiences a more or less considerable increase? There is always the same quantity of electricity; it simply takes less time to traverse the circuit. But if this is the case, it follows that, when the multiplier is replaced by a Weber's electrodynamic dynamometer, the deviation of this instrument must increase. In fact this instrument only differs from an ordinary galvanometer in having the magnet replaced by a moveable helix suspended in the interior of the multiplier by two wires very near each other. The apparatus being so arranged that the current traverses successively the multiplier and the helix, it follows that the action exerted by the multiplier on the helix will be proportional to the *square* of the intensity of the current, and that, if the duration of this current is very small as compared with that of an oscillation of the helix, the elongation obtained will be proportional to the square of the intensity of the current, and inversely as its duration. Hence, if the same quantity of electricity traverses the dynamometer in different spaces of time, and while it lasts the intensity of each current remains constant, the elongation observed (which I assume to be very small) will be inversely as the time which the current will have taken in traversing the dynamometer, or, in other terms, directly as its electromotive force. This elongation is measured very accurately by MM. Poggendorff and Gauss's

method, by means of a small mirror which follows the motion of the helix.

In this instrument the resistance of the air and the currents induced by the terrestrial magnetism are the only forces which tend to deaden the oscillations of the moveable helix when a momentary current has caused it to deviate from its position of equilibrium. It follows that the helix only returns to this position after having made a great number of oscillations. The means by which the amplitude of the oscillations may be appreciably reduced in a sufficiently rapid manner are readily imagined; but to extinguish them completely, much more time is necessary. If each time, before commencing a new experiment, it were necessary to wait until the helix was quite at rest, successive observations would be separated by considerable intervals of time. This difficulty may, however, be easily surmounted. In fact let us suppose that, while the bobbin makes its backward and forward motion, the inducing circuit is opened at the moment at which the bobbin passes by its position of rest, and let us suppose that we obtain a deflection to the left, equal, for example, to u . If care has been taken to note the elongation to the right, u , which has preceded the moment at which the induced current was produced, the velocity of the bobbin at the moment at which it passed its position of rest would be proportional to

$$u - u_1 e^{-nT},$$

e^{nT} being the quantity of which the logarithm represents the logarithmic decrement of the system. It is clearly easy to compare the elongations obtained; it is enough to subtract the quantity $u_1 e^{-nT}$.

It is necessary then to know the value of e^{-nT} ; but it is useless to determine it with a high degree of accuracy, seeing that the quantities u_1 which entered into my experiments were always very small. To obtain it I employed the method of M. Weber (*Electrodynamische Maasbestimmungen*, p. 273), and which is the same as that which Gauss has given in the *Resultate des Magnetischen Vereins im Jahre 1837*; and I convinced myself that so long as u_1 did not exceed five divisions of my scale, we could put

$$e^{-nT} = 1.$$

In the experiments which I am about to describe, the ends of the wire of the dynamometer were in metallic connexion with the two ends of the secondary helix of a Ruhmkorff's apparatus, large size. The current was furnished by four Daniell's elements, the zinc plates of which were 20 centimetres in height and 16 centimetres in breadth. The rupture of the primary current was made alternately in air and in a coal-gas flame. The results I obtained are given in the following Table:—

Position of equilibrium.	Medium in which the rupture was effected.	Elongation observed		Deflection	
		Before the passage by the position of rest.	After the rupture of the inducing circuit.	Observed.	Corrected.
516.4	{ Air	517.8	502.8	13.6	12.2
	{ Flame ...	517.2	502.8	13.6	12.8
	{ Air	517.8	501.3	15.1	13.7
516.1	{ Air	516.4	501.8	14.7	14.0
	{ Flame ...	516.4	501.4	16.1	14.4
	{ Air	517.0	500.0	15.4	15.2
	{ Flame ...	516.5	500.7	16.3	15.0
	{ Air,	516.9	499.8	16.3	15.5
515.9	{ Air	516.1	499.6	16.3	16.1
	{ Flame ...	516.3	499.7	16.2	15.8
	{ Air	516.6	499.9	16.0	15.3
515.9	{ Air	516.3	498.8	17.1	16.7
	{ Flame ...	516.6	498.0	17.9	17.2
	{ Air	516.1	498.1	17.8	17.6
	{ Flame ...	516.1	498.0	17.9	17.7
	{ Air	516.3	498.1	17.8	17.4
515.7	{ Air	516.3	496.9	18.8	18.3
	{ Flame ...	516.1	496.8	18.9	18.5
	{ Air	516.8	496.7	19.0	17.9
	{ Flame ...	516.1	496.9	18.8	18.4
	{ Air	516.1	496.3	19.4	19.0

Taking the mean of these five series of observations, we obtain the following results:—

Deflections produced by the induced current on breaking the circuit in

Air.	Flame.
12.9	12.8
14.9	14.7
15.7	15.8
17.2	17.4
18.4	18.4
Mean . . 15.8	Mean . . 15.8

I have no need to say that I was far from expecting this perfect equality between the two final means.

11. I found some years ago (Poggendorff's *Annalen*, vol. lxxxix. p. 166, and Phil. Mag. S. 3. vol. xix. p. 240) that the striking distance of an induced current was considerably increased when the inducing circuit was broken between the poles of an electro-magnet. I had believed until now that, by breaking contact under these circumstances, the deflection which the current produces in Weber's dynamometer would be also increased. However, the experiments which I have adduced (§ 10) were of a

nature to raise many doubts on this subject. I therefore resolved to make some experiments to ascertain on what I could rely, and I found that *the deviations of the dynamometer do not change when the inducing current is broken between the poles of an electro-magnet.*

12. We have seen on what considerations we depend when we desire to prove that whatever tends to increase the striking distance of the induced current, ought also to tend to increase the deflection which this current produces in a dynamometer. We know that by analogous reasonings it is thought to be shown that the physiological effect of the induced current, as well as the thermal action which it exerts on a metallic wire, ought both to increase when the rupture of the inducing circuit is effected under circumstances favourable to the increase of the striking distance. But having seen that the deflections of the dynamometer do not necessarily depend on the circumstances which favour the striking distance of the induced current, I concluded it was very possible that the physiological and thermal effects were also independent, and that it was for experiment to decide.

To investigate the physiological effect, I employed a small-sized Ruhmkorff's coil. Although I only used a single Daniell's element, yet to render the shock bearable it was necessary to withdraw almost completely the bundle of iron wires. But far from the shock becoming stronger, I found constantly that it diminished in intensity when the rupture took place in a coal-gas flame. I obtained the same results by working with another and less powerful induction coil, and in which I could introduce a greater extent of bundle of iron wire.

An electrical thermometer, constructed according to the plan of M. Riess, served to compare the thermal effects which the induced current produces when the rupture of the inducing circuit takes place successively in air and in the middle of a flame. The apparatus was arranged so as to render it as sensitive as possible. It is almost unnecessary to add that the two ends of the platinum wire with which the apparatus is furnished were in metallic connexion with the two ends of an induced helix. I commenced by using a Ruhmkorff's apparatus, but I soon saw that the induced current was without action on the electro-thermometer, and that I should only succeed by using an apparatus in which the induced wire did not offer such a considerable resistance. I obtained satisfactory results, although the heat developed in the platinum wire was inconsiderable, by making use of an electro-magnet which I employ for experiments on diamagnetism, and which was constructed according to Ruhmkorff's plan (Daguin, *Traité de Physique*, 2nd edit. vol. iii. p. 749). Each of the bobbins consists of three copperwires, one of which served as

induced, the two others as inducing helix. The depressions of the column of liquid are given in the following Table :—

Depressions obtained by breaking the inducing circuit in

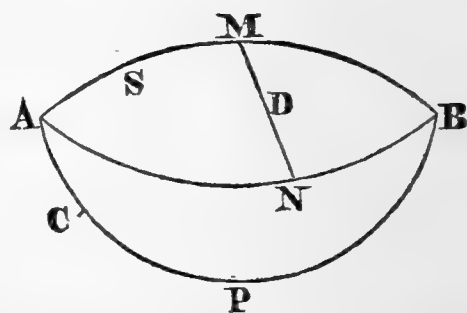
	Air.		Flame.
	0·9		0·8
	1·3		1·3
	0·7		0·7
Mean . .	0·9	Mean . .	0·9

Hence *the mode of breaking contact is without influence on the quantity of heat developed.*

13. I ought especially, from the theoretical point of view, to attach importance to knowing the kind of action which the medium in which the rupture of the inducing circuit is made exerts on the extra current.

With this view I compared the physiological effect which the extra current produces when the circuit is broken in air, to the action which it exerts when the rupture takes place in the middle of a flame. For this experiment I used a plane spiral, consisting of a copper ribbon 130 metres long, 26 millims. wide, and 0·3 millim. in thickness. Holding in each hand a mandril connected with the two ends of the spiral, the *shock was incomparably stronger when contact was broken in a flame than when it was broken in air.* I repeated this experiment with a small-sized Ruhmkorff's apparatus, and I again found that the physiological effect produced by the extra current was stronger when contact was broken in flame than it was when broken in air.

To measure the action which the extra current exerts on the dynamometer, I used a method which depends upon the following theorem. Suppose a system of six conductors united, as shown in the annexed figure. If, in the conductors APB and AM, there are electromotive forces which we shall represent by e and e_1 , the intensity of the current which traverses the conductor MN will be independent of the resistance of the conductor APB and of the electromotive force which resides in it, provided the resistances of the conductors AM, MB, AN, and NB, which we will represent by r_1 , r_2 , r_3 , and r_4 , are in the proportion



$$r_1 : r_2 = r_3 : r_4.$$

In fact, let r_0 be the resistance of the conductor MN, r that of the conductor APB, and let us represent by

i_0	the intensity of the current which circulates in			MN
i	„	„	„	APB
i_1	„	„	„	AM
i_2	„	„	„	MB
i_3	„	„	„	AN
i_4	„	„	„	NB.

We shall have from Ohm's law,

$$i_1 = i + i_3, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (a)$$

$$i_1 = i_2 + i_0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (b)$$

$$i_3 = i_0 + i_4; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (c)$$

and from Kirchhoff's principle,

$$i_0 r_0 + i_1 r_1 + i_3 r_3 = e_1, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (d)$$

$$i r + i_1 r_1 + i_2 r_2 = e + e_1, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (e)$$

$$i_2 r_2 + i_4 r_4 - i_0 r_0 = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (f)$$

Eliminating i_2 and i_3 ,

$$i_0 + i - i_1 + i_4 = 0, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (g)$$

$$i_0 r_0 - i r_3 + i_1 (r_1 + r_3) = e_1, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (h)$$

$$-i_0 r_2 + i r + i_1 (r_1 + r_2) = e + e_1, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (i)$$

$$-i_0 (r_0 + r_2) + i_3 r_3 + i_4 r_4 = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (k)$$

Eliminating i_4 from the equations (g) and (k), we get

$$-i_0 (r_0 + r_2 + r_4) - i r_4 + i_1 (r_2 + r_4) = 0. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (l)$$

The equations (h), (i), and (l) give by the elimination of i and i_1 ,

and replacing r_4 by its value $\frac{r_2 r_3}{r_1}$,

$$i_0 = \frac{e_1 r_2}{r_0 (r_1 + r_2) + r_2 (r_1 + r_3)}.$$

Thus the last member of this equation contains neither e nor r .

Let us suppose now that we have at P a voltaic apparatus, at S a spiral or any other apparatus in which an extra current could be induced, and at D a Weber's dynamometer. In this case e will represent the electromotive force which operates at P, e_1 that which is induced in S, and r_1 the resistance of the spiral and of the conductors which join its ends to the knots A and M. If we break the primary current at C, for instance, r will represent the resistance of the voltaic apparatus increased by the resistance which the primary current meets in its passage in P and B and P and A, and which is variable, seeing that the resistance of the voltaic arc formed at C is comprised in it. The deflection which the moveable helix of the dynamometer will undergo, provided it is not too large, will be proportional to the quantity

$$\int_0^T \left[\frac{e_1 r_2}{r_0 (r_1 + r_2) + r_1 (r_1 + r_3)} \right]^2 dt,$$

T representing the duration of the extra current. Now, operating as I have described, the *rupture of the primary current in air, in a flame, and between the poles of an electro-magnet, always gave rise to the same deflection of the moveable helix.*

14. I described some years ago (Pogg. *Ann.* vol. lxxxi. p. 168, Phil. Mag. S. 3. vol. xix. p. 240), the increase in length which induction sparks undergo when the primary current is broken between the poles of an electro-magnet. I believe that, by analogous considerations, an explanation might be given why the striking distance of an induced current increases when the molecules of the gas or of the liquid in which contact is broken are endowed with a certain velocity. In fact, when the circuit is opened, the current is not interrupted so suddenly as is usually supposed. At the part at which the circuit is broken, a more or less intense voltaic arc is produced, which continues until the electrodes have reached a certain distance. It is clear that this arc, though offering a more and more considerable resistance, allows the current to pass from one electrode to the other. It is also clear that, if the gaseous or liquid molecules in which the voltaic arc is formed are endowed with a certain velocity, they will communicate part of their motion to the particles which constitute the voltaic arc. It will follow that this arc will not remain so long, and that therefore the velocity of the variation in the intensity of the induced current will increase. But the electromotive force of the induced current, as well as its striking distance, are quite proportional to this velocity of variation. Then a certain degree of velocity imparted to the molecules of the gas or of the liquid in which the rupture of the inducing circuit is effected ought to increase the striking distance of the induced current. But, it will doubtless be objected, the physiological effect, the thermal effect, as well as the action on the dynamometer, why will they not increase at the same time? I admit that to these questions, the number of which might be increased, I cannot give a satisfactory answer. I merely beg to observe that Ohm's laws are tacitly allowed to be applicable to induced currents obtained by opening or closing an inducing circuit. But that is an entirely gratuitous supposition. Ohm's laws are only applicable to circuits in which a state of electrodynamic equilibrium has been established. But no one will aver that this is the case with the circuits in question.

With regard to the specific action exercised by different gases, I do not think we can dream of accounting for it in the present state of the science.

Leyden, September 1862.

XXXV. *A Mathematical Discussion of the properties of the Air Thermometer with respect to its fitness for determining the instantaneous changes of Temperature of its contained Air, arising from sudden changes of Density.* By Professor POTTER, A.M.*

THE properties of the air thermometer, which it is proposed to discuss in the present paper, are of considerable scientific interest. M. Poisson† had found that the theory of sound, upon the hypothesis of the infinite divisibility of matter, required that in the sudden condensation of a gas the temperature should rise 1° Centigrade for every $\frac{1}{116}$ th part condensation, and fall the same quantity for every $\frac{1}{116}$ th part rarefaction. This received no experimental support until MM. Clement and Desormes‡, in the year 1819, contrived an experiment with a receiver (*ballon*) furnished with pipes and stopcocks, leading, the one to an air-pump, and the other to the external air, and other pipes forming barometer gauges (*manomètres*) of mercury and water. The air in the receiver, after a slight exhaustion, was allowed to communicate with the external air, which thus re-entered suddenly, and then the communication with the external air was cut off. By observing the movements of the liquids in the gauges, it was thought that the effect of the heat arising from the sudden condensation was exhibited by them, and that the apparatus thus became a sensitive air thermometer.

In the *Mécanique Céleste*, vol. v. p. 125, published in 1825, M. Laplace describes the method of the experiments of MM. Gay-Lussac and Welter with a like balloon and manometer or gauge, only employing condensations where MM. Clement and Desormes had used rarefactions, as well as precautions for increased accuracy. The method is still that of a sensitive air thermometer.

Mr. Meikle also employed the same method, with a liquid for the gauge much lighter than water, the vapour of which in the receiver might be an objection.

In my paper in the *Philosophical Magazine* for January last, I have described some of my own experiments with Newman's air-pump and barometer-gauge on the same principle. There is a remarkable accordance in all the results which have been obtained by the different experimenters when the rarefaction or condensation was small; they do not differ more than might have been expected from different forms of apparatus and in different hands, and are not far from what is required in Poisson's theory of sound. The results for greater rarefactions were, however, in my experiments, very different from those cal-

* Communicated by the Author.

† *Traité de Mécanique*, vol. ii. p. 643.

‡ *Journal de Physique* for November 1819.

culated by Poisson's rule. I was, I believe, the first to point out, in my paper in the Philosophical Magazine for September 1853, that there is a fundamental objection to the method, in whatever form it is used, namely, that it is a dynamical experiment, and the dynamical result is neglected. In the paper I say, "the effect of the momentum of the moving column of water in the gauge in passing from one position of equilibrium to another has been attributed to the effect of heat developed in the sudden condensation."

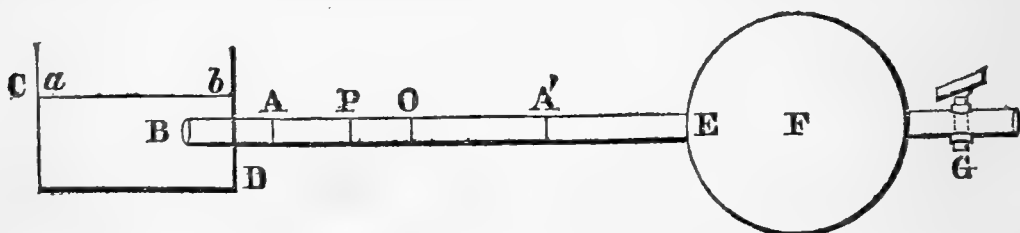
It is clear that the discussion of the dynamical problem is needed before we can judge of the thermometric results; and the *desirable verification* of Poisson's formula which MM. Clement and Desormes invented may turn out after all to be nothing more than *fudge verification*, although universally adopted in our higher treatises on hydrostatics and hydrodynamics as valid verification.

In the forms of the experiment which have been used, the mathematical discussion is very simple if we consider only the liquid in the tube of the gauge or manometer to be subject to pressures which communicate motion to it; but we must remember that motion is also produced in the liquid of the cistern; and this we cannot discuss for want of the requisite data.

Taking the simplest case first,

Prop. *To determine the motion of the liquid in the tube of an air thermometer arising from a sudden rarefaction of the air in the receiver when the tube is horizontal.*

Let C D be the cistern containing the liquid mercury, water, or spirit; F the bulb or receiver of the thermometer, containing



air, as well as its tube E A B as far as A. The tube passing into the cistern below the surface of the liquid *ab* as in the figure, let the space at the first position of equilibrium which is filled with the liquid be A B.

Then if the bulb has a pipe furnished with a stopcock G, and is connected by it with an air-pump or other exhausting apparatus, and a portion of air is suddenly withdrawn and the stopcock G then closed, the liquid A B will be put into motion, since the pressures at its extremities A and B will be then unequal, and the pressure of the atmosphere together with that of the liquid in the cistern will cause an additional portion of liquid to enter the tube, keeping it filled. Let O be the place where the

extremity of the liquid column will rest when the equilibrium is restored, and $OA=a$, $AB=b$.

If P is the position of the extremity of the column at any time t , let $OP=x$.

Let α = perpendicular section of the tube,
 ρ = density of the liquid in the cistern and tube,
 then the mass in motion at the time t is $\rho\alpha \times BP$

$$= \rho\alpha(a+b-x).$$

The moving force on the column BP is the difference of the pressures on its ends at B and P , of which that at B is constant, whilst that at P is variable and given by Boyle's law whilst we neglect considerations of heat or cold developed, and the adhesion of the liquid to the tube.

Let V be the volume of the receiver and tube up to the point O ; then the volume to the point P is $V+\alpha x$, and

$$\frac{\text{the elastic force of contained air in volume FO}}{\text{the elastic force of contained air in volume FP}} = \frac{V+\alpha x}{V}.$$

Let p = constant pressure on a unit of area of the atmosphere and the liquid in the cistern at B , and therefore $p\alpha$ = pressure on the surface of the end of the column at B as well as at A , and O when there is equilibrium. We have the moving force on the column when the surface is at P

$$= p\alpha - \text{pressure due to the elastic force of the contained air on the surface at } P,$$

$$= p\alpha - p\alpha \cdot \frac{V}{V+\alpha x}$$

$$= \frac{p\alpha x}{V+\alpha x},$$

which varies as x very nearly, since αx is very small compared with V .

From these we have the accelerating force acting upon the column of liquid $BP = - \frac{d^2x}{dt^2}$

$$= \frac{\text{the difference of the pressures at } B \text{ and } P}{\text{mass of the liquid in the column } BP}$$

$$= \frac{p\alpha x}{(V+\alpha x)\rho\alpha(a+b-x)},$$

$$= \frac{p}{\rho V} \cdot \frac{x}{a+b-x} \text{ very nearly, since } \alpha x \text{ is very small compared with } V.$$

Multiplying each side by $2 \cdot \frac{dx}{dt}$ and integrating, we have

$$\frac{dx^2}{dt^2} = \frac{2p}{\rho V} \cdot \int \left(1 - \frac{a+b}{a+b-x}\right) dx;$$

or putting $v =$ the velocity of the surface at P $= \frac{dx}{dt}$, then

$$v^2 = \frac{dx^2}{dt^2} = \frac{2p}{\rho V} \{x + (a+b) \log_e (a+b-x)\} + C;$$

and the velocity is 0 at A when $x=a$;

$$\therefore 0 = \frac{2p}{\rho V} \{a + (a+b) \log_e (b)\} + C.$$

Subtracting, we have the corrected integral

$$v^2 = \frac{2p}{\rho V} \left\{ (a+b) \log_e \left(\frac{a+b-x}{b} \right) - (a-x) \right\}.$$

From the correction of the integral, this will always give the velocity $v=0$ when $x=a$; and there will be another value of x when $v=0$, as A'O, to be determined by the equation

$$\log_e \left(\frac{a+b-x}{b} \right) = \frac{a-x}{a+b},$$

of which the value of the roots can be found by trial and successive approximations in terms of given values of a and b . These give as follows the values of x when $v=0$.

When

a is small compared with b , the roots are $x = +a$ and $x = -a$;

$a=b$, „ „ $x = a$ „ „ $x = -1.513a$;

$a=2b$, „ „ $x = a$ „ „ $x = -1.856a$;

$a=3b$, „ „ $x = a$ „ „ $x = -2.116a$;

we see that if there were no adhesion of the liquid to the tube, and no heat or cold developed, there would be unsymmetrical oscillations of the surface at P about the point O, similar to what we find in certain other dynamical problems.

Next, taking the case of the vertical barometer gauges or manometers, as used in all the experiments, together with the adhesion as proportional to the adhering surfaces of the liquid and the tube, we have the following proposition:—

Prop. *To determine the motion of the liquid in the vertical gauge of an air thermometer, taking into account the adhesion of the liquid to the tube, when it arises from sudden changes of density of the contained air.*

Let F be the ball or receiver of the thermometer furnished with a pipe and stopcock G, by which a portion of the air may be suddenly removed and the stopcock closed.

Let EB be the vertical tube of the gauge dipping into the cistern CD which contains the liquid of the gauge.

Let A be the level of the liquid inside the tube in equilibrium before the portion of the air is removed through G, and O the new position of equilibrium after that removal, and the volume of the contained air then V.

Let P be the surface of the column of the liquid PB at any time t , and $OP = x$, $OA = a$, $AB = b$. The upper surface of the liquid will oscillate about O, and may attain some maximum height OA' , when the velocity will be again nothing.

Let r be the radius of the tube, and α its perpendicular section, or $\pi r^2 = \alpha$; let ρ = density of the liquid of the gauge. Then, as in the former case, the mass of the column $PB = \rho\alpha(a + b - x)$,

the elastic force of the air in the receiver and pipe to P $\frac{V}{V + \alpha x}$.

If p is the pressure of the atmosphere on a unit of area when the height of the barometer formed of the liquid is h , then the elastic force of the air filling the receiver and tube to O

$$= p - g\rho \cdot OA \\ = g\rho(h - a),$$

and the elastic force in the volume to P $= g\rho(h - a) \frac{V}{V + \alpha x}$.

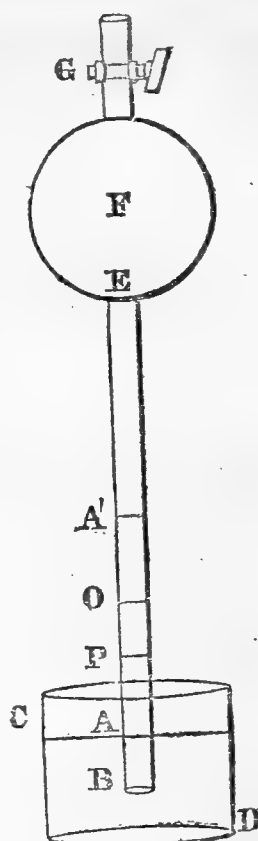
The moving force acting on the column PB = pressure of the atmosphere on the area α — the weight of the column PA of the liquid — the elastic force of the air contained in the receiver and the tube to P, acting on the area α ,

$$= g\rho\alpha \left\{ h - (a - x) - (h - a) \frac{V}{V + \alpha x} \right\} \\ = g\rho\alpha x \left(1 + \frac{\alpha(h - a)}{V + \alpha x} \right).$$

Let V now $= \alpha h'$, where h' will be very large in the experiments compared with a and x , and we may put

$$\frac{\alpha(h - a)}{V + \alpha x} = \frac{\alpha(h - a)}{\alpha h' + \alpha x} = \frac{h - a}{h' + x} = \frac{h}{h'}$$

nearly when a and x are small.



Again, the adhesion between the surfaces of the liquid and the tube, which is proportional to those surfaces,

$$= k \cdot 2\pi r(a+b-x),$$

if k is the adhesion of unity of the surfaces.

Taking this adhesion as a retarding pressure, and putting for k its value in terms of the pressure at a depth z of the liquid, as in hydrostatics, or

$$k = g\rho z,$$

and also putting $\frac{z}{r} = n$, which must be a known number, we have

$$-\frac{d^2x}{dt^2} \cdot \rho\alpha(a+b-x) = g\rho\alpha x \left(1 + \frac{h-a}{h'+x}\right) - 2\pi rk(a+b-x);$$

whence, since $\alpha = \pi r^2$,

$$\begin{aligned} -\frac{d^2x}{dt^2} &= \frac{gx}{a+b-x} \left(1 + \frac{h-a}{h'+x}\right) - \frac{2\pi g z r}{\alpha} \\ &= g \left\{ \left(1 + \frac{h-a}{h'+x}\right) \frac{a+b-(a+b-x)}{a+b-x} - 2n \right\}; \end{aligned}$$

$$\therefore \frac{d^2x}{dt^2} = g \left(2n + \left(1 + \frac{h-a}{h'+x}\right) - \left(1 + \frac{h-a}{h'+x}\right) \frac{a+b}{a+b-x} \right).$$

Multiplying each side by $2 \frac{dx}{dt}$ and integrating, we have

$$\begin{aligned} \frac{dx^2}{dt^2} = v^2 &= 2g \int \left\{ 2n + \left(1 + \frac{h}{h'}\right) - \left(1 + \frac{h}{h'}\right) \frac{a+b}{a+b-x} \right\} dx, \\ &= 2g \left\{ \left(2n + \left(1 + \frac{h}{h'}\right)\right)x + \left(1 + \frac{h}{h'}\right)(a+b) \log_e(a+b-x) \right\} + C, \end{aligned}$$

and $v=0$ when $x=a$;

$$\therefore 0 = 2g \left\{ \left(2n + \left(1 + \frac{h}{h'}\right)\right)a + \left(1 + \frac{h}{h'}\right)(a+b) \log_e(b) \right\} + C,$$

and \therefore generally,

$$v^2 = 2g \left\{ \left(2n + \left(1 + \frac{h}{h'}\right)\right)(x-a) + \left(1 + \frac{h}{h'}\right)(a+b) \log_e \left(\frac{a+b-x}{b} \right) \right\}.$$

From the correction of the integral we have in all cases $v=0$ when $x=a$; and the other value of x , where v is again $=0$, is obtained by trial and successive approximations from the equation

$$\log_e \left(\frac{a+b-x}{b} \right) - \left(1 + \frac{2n}{1 + \frac{h}{h'}} \right) \left(\frac{a-x}{a+b} \right) = 0.$$

It will be seen that this equation differs from the one correspond-

ing to it of the former proposition only in having the coefficient $\left(1 + \frac{2n}{1 + \frac{h}{h'}}\right)$ in place of 1.

As the values of n and h' are not given for any experiments yet published, we are not able to make calculations for comparison with them; but can, by taking different values of the coefficient $\left(1 + \frac{2n}{1 + \frac{h}{h'}}\right)$, see that the experiment is a dynamical

one in the first instance. There is not, however, oscillation in every case, as might have been foreseen; for when a is small compared with b , the adhesion is more than the active moving force, and no motion ensues, the equation giving no other admissible value for x when $v=0$ than $x=a$.

For certain values of a with respect to b , and of the coefficient, the two values of x when $v=0$ are both positive, or A' lies below 0 as well as A . For a certain value of a and of the coefficient, the second value of x for $v=0$ is $x=0$; which case I have calculated for the particular relation of $a=2b$, which may be tried when the coefficient has been determined to apply to that particular case.

When $\left(1 + \frac{2n}{1 + \frac{h}{h'}}\right) = 1.01$, or $n = .005 \left(1 + \frac{h}{h'}\right)$,

if $a = b$, then the roots are $x = +a$, and $x = -1.455a$;

$a = 2b$, „ „ $x = a$, „ $x = -1.805a$;

$a = 3b$, „ „ $x = a$, „ $x = -2.065a$.

When $\left(1 + \frac{2n}{1 + \frac{h}{h'}}\right) = 1.1$, or $n = .05 \left(1 + \frac{h}{h'}\right)$,

if $a = b$, then the roots are $x = +a$, and $x = -.994a$;

$a = 2b$, „ „ $x = a$, „ $x = -1.400a$;

$a = 3b$, „ „ $x = a$, „ $x = -1.661a$.

When $\left(1 + \frac{2n}{1 + \frac{h}{h'}}\right) = 1.5$, or $n = .25 \left(1 + \frac{h}{h'}\right)$,

if $a = b$, the roots are . . . $x = +a$, and $x = +.266a$;

$a = 2b$, „ „ $x = a$, „ $x = -.257a$;

$a = 3b$, „ „ $x = a$, „ $x = -.530a$;

$a = 6b$, „ „ $x = a$, „ $x = -.992a$;

$a = 9b$, „ „ $x = a$, „ $x = -1.270a$.

For the case when $x=0$ or A' coincides with O , when $a=2b$, we find $\left(1 + \frac{2n}{1 + \frac{h}{h'}}\right) = 1.64792$, or $n = .32396 \left(1 + \frac{h}{h'}\right)$ nearly; and then if

$a=2b$, the roots are $x = +a$, and $x=0$;

$a=6b$, „ „ $x = a$, „ „ $x = -.717a$;

$a=9b$, „ „ $x = a$, „ „ $x = -.978a$.

With respect to my own experiments, I have found a note that the tube of the barometer-gauge passed to $\frac{1}{8}$ inch below the surface of the mercury, and that there was a depth of .85 inch of mercury below it in the cistern; but the radius of the tube of the gauge is not stated; and even if the value of the dynamical adhesion of the mercury to the glass were known, the value of n would remain unknown.

The measures on which the calculations for the degree of cold produced, from Amontons's formula, were made in my paper of January 1862, were as follows:—

When $a=2.01$ in.,	the mercury rose to 2.64 in.,	and the calc. cold	$= 11^{\circ}.59$;
$a=6.20$	„ „ 7.60	„ „	$= 29.4$;
$a=6.18$	„ „ 8.18	„ „	$= 43.2$;
$a=9.20$	„ „ 11.25	„ „	$= 50.5$.

With b so small as $\frac{1}{8}$ inch, the small extent of the oscillations here shown proves that the adhesion must have been much greater than supposed in any of the preceding computations, even if no cold arose from the rarefaction.

The problem is evidently a dynamical one before it becomes a thermometrical one; and if we apologize for the chemists who invented or employed the experiment, what must we say for the mathematicians who have accepted it as a purely thermometric experiment?

XXXVI. *On the Absorption and Radiation of Heat by Gaseous Matter.*—Second Memoir. By JOHN TYNDALL, F.R.S., Professor of Natural Philosophy in the Royal Institution*.

§ 1. **I**STRUMENTS.—The apparatus made use of in this inquiry is the same in principle as that employed in my last investigation†. It grew up in the following way:—A tube was first procured to receive the gases through which radiant heat was to be transmitted; but it was necessary to close the ends of

* From the Philosophical Transactions, Part I. for 1862, having been read at the Royal Society January 30, 1862.

† Philosophical Transactions, 1861; and Phil. Mag. vol. xxii. p. 169.

this tube by a substance pervious to all kinds of heat, obscure as well as luminous. Rock-salt fulfils this condition; and accordingly plates of the substance an inch in thickness, so as to be able to endure considerable pressure, were resorted to. In the earliest experiments a cube of boiling water was placed before one end of this tube, and a thermo-electric pile connected with a galvanometer at the other; it was found that if the needle pointed to any particular degree when the tube was exhausted, it pointed to the same degree when the tube was filled with air. Thus tested, the presence of dry air, oxygen, nitrogen, or hydrogen had no sensible influence on the radiant heat passing through the tube.

In some of these trials the needle stood at 80° , in some at 20° , and in others at intermediate positions. I reasoned thus:—The quantity of heat which produces the deflection of 20° is exceedingly small, and hence a minute fraction of this quantity, even if absorbed, might well escape detection. On the other hand, the quantity of heat which produces the deflection of 80° is comparatively large, but then it would require a large absorption to move the needle even half a degree in this position. A deflection of 20° is represented by the number 20, but a deflection of 80° is represented by the number 710. While pointing to 80, therefore, an absorption capable of producing a deflection of 15 or 20 degrees on the lower part of the scale, would hardly produce a sensible motion of the needle. The problem then was, to work with a copious radiation, and at the same time to preserve the needle in a position where it would be sensitive to the slightest fluctuations in the absolute amount of heat falling upon the pile.

This problem was finally solved by converting the pile into a differential thermometer. Its second face was exposed, and a second source of heat was placed in front of that face. A moveable screen was interposed between the two, by the motion of which the same amount of heat could be caused to fall upon the posterior surface of the pile as that received from the experimental tube by its anterior surface. When this was effected, no matter how high the previous deflection might be, it was completely neutralized, and the needle descended to zero.

Supposing this equality to have been established when the tube was exhausted, it is manifest that any gas, capable of absorbing even an extremely small proportion of radiant heat, would, if introduced into the tube, destroy the equilibrium of both sources. The second source of heat would now predominate, and a deflection of the galvanometer needle would be the consequence. The magnitude of this deflection would depend on the quantity of heat cut off by the gas, and, properly reduced, it became a strict measure of the absorption.

But in these experiments my first source of heat stood at some distance from the anterior end of the tube, and the heat, previously to entering the latter, had to cross a space of air which was not the subject of examination. This air-space I wished to abolish, so as to allow the calorific rays to enter the gas with all the qualities which they possessed at the moment of emission. I first thought of soldering the end of the experimental tube direct to the radiating surface, thus allowing the air to come into direct contact with the source. But it immediately occurred to me that the introduction of cool air into the tube would lower the temperature of the source, and that I could never know how far the indication of my galvanometer under such circumstances could be regarded as a true effect of absorption; hence I abandoned the idea of bringing the gases into contact with the radiating surface.

Instead of this arrangement an independent tube, 8 inches long, and of the same diameter as the experimental tube, was soldered on to the radiating plate. By means of a screw joint, the free end of this tube was connected air-tight with the experimental tube. Thus a chamber, from which the air could be removed, was introduced between the first plate of salt and the radiating surface. Two objects were thus secured: first, my source of heat was withdrawn from the action of irregular currents of air; and secondly, the radiant heat entered the tube unchanged in quality save the infinitesimal change due to its passage through the diathermic salt.

To save the trouble and expense of a new Plate, I will ask permission to refer in this memoir to the Plate (Pl. III. vol. xxii.) made use of in my last; a verbal reference will in most cases be sufficient to indicate the changes recently introduced. S S' it will be remembered, represented the experimental tube, which was then made of brass polished within. Such a tube could not be used for any gases or vapours capable of attacking brass; and though I combated this difficulty, to some extent, by blackening the tube within, I could never feel at ease regarding the action of the gases upon the blackening substance. Many gases, moreover, present great difficulties on account of their affinity for atmospheric moisture. Hydrobromic and hydrochloric acid, for example, form dense fumes in the air; and however carefully they might have been dried, I should have been reluctant to base any inference on their deportment without actually having them under my eyes during experiment.

The brass tube, then, which stretched from S to S' in the figure is now replaced by one of glass, 2 feet 9 inches long, and 2.4 inches in diameter. The source of heat in my last-published inquiry was the cube of hot water C; but glass being far inferior

to brass in reflecting power, I was unable with this source to bring out with due force the vast differences existing between various kinds of gaseous matter. I therefore had a copper hood constructed, and united by brazing with a tube 8 inches long, which was destined to form the vacuous chamber in front of the first plate of rock-salt. To heat the copper plate, a lamp formed on the principle of Bunsen's burner was made use of. The gas passed upwards by four hollow columns, each perforated for the admission of air. The mixture of air and gas escaped from these columns into a chamber shaped like the frustum of a cone, and over this chamber was placed a shade of thin sheet-iron, the top of which was narrowed to a slit one-eighth of an inch wide and 2 inches long. From this slit the mixture of gas and air issued, and formed upon ignition a sheet of flame. This was caused to glide along the back of the copper plate before referred to, which was thereby raised to a temperature of about 270° C. To preserve this source constant was one of the greatest difficulties of the investigation; for the slightest agitation of the surrounding air, or the slightest flickering of the flame itself, was sufficient to disturb the steadiness of the galvanometer and to render experiments in delicate cases impossible. The flame was surrounded by screens of pasteboard, these being again encompassed by towels, through the meshes of which the flame was fed; a suitable chimney produced a gentle draught and carried off the products of combustion; the rhythmic jumping of the flame itself was destroyed by screens of wire-gauze; in short, six weeks' practice was required to master all the difficulties of this portion of the apparatus. The "compensating cube" C', the double screen H, and the thermo-electric pile P remain as before. They are exposed in the figure, but during the experiments they were surrounded by a close hoarding, all the chinks of which were stuffed with tow, so as to protect the cube and pile from the disturbing action of the air. To protect the anterior plate of rock-salt from the heat which might have been conducted to it from the source, the front chamber passed as before through a vessel V in which a current of cold water, constantly renewed, was caused to circulate.

§ 2. *Experiments.*—On two points I wished to set my mind at rest previous to starting on my vacation tour this year. These were the absorption of chlorine gas and of ozone. On the 16th, 17th, and 18th of June I experimented on these two substances, and satisfied myself that chlorine was far outstripped by many colourless gases, and that ozone had a power of absorption very much greater than common oxygen.

The work was resumed on the 12th of September, and my first care was to examine whether my published experiments on

moist and dry air stood the test of repetition. Professor Magnus had experimented on dry air and on air saturated with moisture, and found that the presence of the moisture had no influence on the absorption. I, on the contrary, had previously found, and stated, that dry air had only a small fraction of the absorptive energy of the same air when even partially saturated. I commenced my researches in September with a few experiments on this subject.

Half an atmosphere of the undried air of the laboratory, admitted directly into the tube, cut off an amount of heat which produced a deflection of 30 degrees.

My drying-apparatus at this time consisted of a U-tube filled with fragments of pumice-stone wetted with sulphuric acid. Associated with this was a similar tube filled with like fragments, but moistened with caustic potash solution to remove the carbonic acid of the air.

The air of the laboratory, passed through both these tubes in succession, till a tension of 15 inches was attained, gave a deflection of 26 degrees.

This result surprised me, showing, as it seemed to do, a very close agreement between dry and moist air. On examining the drying-tubes, however, I found that by a mistake of arrangement the air had entered the sulphuric-acid tube first, and passed straight from the potash into the experimental tube, thus partially reloading itself with moisture after it had been dried.

The air was now sent through both tubes, commencing with the potash: the deflection fell instantly to less than 5 degrees. Hence this experiment showed the absorption due to the moisture and carbonic acid of the air to be more than six times as great as that of the atmosphere itself. It will presently be seen that the difference here stated falls far short of the truth.

The potash and sulphuric acid were now abandoned, and the air was dried by passing it through a U-tube filled with fragments of chloride of calcium, which had lain in the tube for some months. The deflection produced by air thus dried was 40 degrees; that is to say, 10 degrees more than that produced by the undried air.

This result, and many others of a similar nature, were due to the imperfection of the chloride of calcium. I think chemists ought to be very cautious in the use of this substance as a drying agent. When pure and newly fused it may answer for this purpose, but when old it yields an impalpable powder, which proved in the highest degree perplexing to me in my first experiments. It is generally found, I believe, that a drying-tube of sulphuric acid gains more in weight than one of chloride of calcium, and from this it has been inferred that the quantity of moisture

taken up by the former is greater than that taken up by the latter. The difference, however, may really be due to the mechanical carrying away of a portion of the chloride by the current of air.

On the 13th of September these experiments were resumed. The dry air then gave a deflection of less than 2 degrees; the air direct from the laboratory caused, in one experiment, the needle to move from 20 degrees on one side of zero to 28 on the other. In a second experiment the undried air caused the needle to move from 18° on one side of zero to 32° on the other.

Experiments made on the 17th entirely corroborated this result. Three successive trials made the action of the undried air of the laboratory 29°, 31°, and 30° respectively, the deflection produced by the dried air being less than a single degree. On this day, therefore, the action of the aqueous vapour of the air was at least thirty times that of the air itself.

Almost every week-day during the last four months, experiments similar to the above have been executed, and in no case have I observed a deviation from the result that the absorptive action of the aqueous vapour of the air is quite enormous in comparison with that of the air itself. Further on I will give an array of figures illustrating this point.

As I became more and more master of my apparatus, and more acquainted with the precautions necessary in delicate cases, the absorption of air and the elementary gases diminished more and more. I was induced to abandon the use of pumice-stone as well as of chloride of calcium, and to construct my drying-apparatus in the following way. The internal portion of a massive block of pure glass was pounded to small fragments in a mortar; these were boiled in pure nitric acid, and afterwards washed several times with distilled water so as to remove all trace of the acid. They were then dried, afterwards moistened with pure sulphuric acid, and introduced by means of a funnel into a U-tube. The funnel was necessary to preserve the neck of the tube from all contact with the acid, the least action of which upon the corks used to close the tube was sufficient to entirely vitiate the experiments. At the top of each arm of the U-tube a quantity of dry fragments of glass was placed, so that if any dust or particles from the cork or sealing-wax chanced to reach the interior they fell upon the dry glass.

Similar precautions were taken with the caustic-potash tube. Pure white marble was pounded to fragments and subjected to the action of a dilute acid, which removed the outer surface of the fragments. These were afterwards washed in distilled water and dried, then moistened with pure caustic potash, and introduced into the U-tube in the manner already described. It was

sometimes necessary to perform this operation daily, and never on any occasion have I used tubes to dry a feeble gas which had been previously used to dry a powerful one.

In the present communication I shall have to touch upon many subjects which for want of time I have been unable to develop. The following is an example of these. Choosing a day of suitable temperature and moisture—a day on which the human breath shows no signs of precipitation—the action of the substances expired from the lungs may be most strictly determined by our apparatus. By breathing directly into the experimental tube, the action produced by the sum of the products of respiration might be accurately measured; by breathing through the sulphuric-acid tube, the moisture of the breath would be withdrawn, and the difference between the action then observed and the former action would give that of the carbonic acid. In this way the products of respiration might be estimated singly, and the influence of various kinds of food and drink, or of physical exertion, on the respiration might be investigated in a manner hitherto unthought of.

I have to record the following experiments only in connexion with this subject. Placing a suitable tube between my lips, I filled my lungs with air; a stopcock which was interposed between me and the experimental tube being partially opened, I breathed through it slowly into the latter until the mercury gauge of the pump was depressed 15 inches. I had, at the time, two assistants, C. A. and R. C., and they subsequently breathed into the experimental tube the same quantity as myself. In the following Table the absorption produced by the breath of each is stated:—

Action of the Products of Respiration on Radiant Heat.

Initials of person's name.	Absorption.	Initials of person's name.	Absorption.
J. T.	62	J. T.	59
J. T.	62	R. C.	63
R. C.	66	C. A.	62
R. C.	68	J. T.	60·5
J. T. again	59		

The absorption of dry air on the day that these results were obtained was found to be 1. *The same dry air inhaled, underwent a chemical change which augmented its absorptive energy at least 60 times.* I give this as a minor limit, and will not say how much I regard it as falling short of the truth.

The day afterwards the following results were obtained, the same amount as before being exhaled:—

Initials.	Absorption.
J. T.	56
R. C.	62
J. T.	56
R. C.	59

In all cases R. C., who is the smallest and least robust man of the three, appeared to have the advantage. I will only add a few results obtained on the 6th of October, the quantity of air expired on the occasion depressing the mercurial column 5 inches.

Initials.	Absorption.
J. T.	33·5
R. C.	35
R. C. After half a glass of Trinity Audit Ale	41
Again	35
After a teaspoonful of brandy	35
After chewing and swallowing a small quantity of onion	40

After taking the ale and brandy my assistant washed his mouth and gargled his throat several times with cold water. I give these results merely as illustrative of one of the numerous applications of the apparatus. In all the experiments the tube remained perfectly transparent throughout, and, on pumping, the needle in each case returned accurately to zero.

§ 3. In my last paper I brought the fact somewhat prominently forward that the elementary bodies which I had then examined were far less hostile to the passage of the longer undulations than the compound ones; and I founded at the time certain theoretic considerations on this fact*. I was desirous this year to extend the experiments to one or two of the coloured gases and vapours, and on the 20th of September resumed my experiments on chlorine. This gas is itself highly coloured, and of a specific gravity of 2·45; one of its compounds, hydrochloric acid, is quite transparent, and of specific gravity of only 1·26. Does the act of combination with hydrogen which renders chlorine gas more transparent to light render it also more transparent to heat? Chlorine prepared from hydrochloric acid and peroxide of manganese, and dried by passing it through sulphuric acid, was admitted into the tube till it depressed the mercury gauge 21 inches; the absorption of the gas was expressed by the number 44.

Hydrochloric acid was admitted till it depressed the gauge 19 inches; the absorption was 68. This experiment indicates that transparency to light and opacity to heat accompany the same act of chemical union.

* Phil. Mag. vol. xxii. p. 169.

The following results were afterwards obtained. I may remark that a subsidiary gauge was used, so as to prevent the destructive gases from entering the air-pump.

	Absorption.
Chlorine 15 inches	32
Chlorine 14 inches	30
Chlorine 14 inches	30
Hydrochloric acid 14 inches	47
Chlorine again	30
Hydrochloric acid	56

In all cases the effect of the compound gas was found to exceed that of the elementary one; so that *the chemical change which renders chlorine more transparent to light renders it more opaque to obscure heat.*

Great care is required in experiments on hydrochloric acid, and great care was bestowed on the above. Previously to the introduction of the gas the experimental tube was filled with perfectly dry air, so as to leave a perfectly dry residue on exhaustion. The gas was allowed to stream through the drying-tube until all traces of air were expelled; then a joint was suddenly broken, and the retort was connected with the experimental tube. The gas thus passed directly from the retort through the drying-apparatus into the vacuum. It was difficult to avoid sending in with the gas a few particles of moisture; but these, if such existed, appeared to be dissipated by the dynamic heating of the gas on entering the tube, and kept dissipated by the flux of heat passing through it. At all events the closest scrutiny could detect no trace of mist or turbidity within the tube; it was perfectly transparent throughout. The chlorine, on the contrary, was intensely coloured.

I made many experiments with chlorine which had been collected over water, but something (what, I know not yet) appeared to be in all cases carried along with the gas from the water into the tube, which materially augmented its absorption.

The above experiments were made in the early part of this inquiry, and before I had become aware of all the peculiarities of my apparatus. Subsequent experiments reduced in some degree the absorption both of chlorine and hydrochloric acid. Very careful experiments made on the 29th of October gave the following absorption for these two gases, at a tension of 30 inches:—

Chlorine	39
Hydrochloric acid	53

The chlorine and hydrochloric acid were removed from the experimental tube in the following manner:—A cock and con-

necting piece were attached to one end of the experimental tube, and from them a length of india-rubber tubing led to the flue of the laboratory stove. A gas-holder of air was put in connexion with the other end of the experimental tube, a system of drying-tubes intervening between the latter and the holder. By a slight water-pressure a stream of dry air was carried gently through the tube to the flue, and in this way the gases, which if pumped out would have injured the pistons, were speedily removed. As the dry air replaced the gases, the needle gradually descended to zero, its arrival there being indicative of the complete displacement of the gas. The perfect dryness of the air thus made use of was beautifully proved. Had the air contained moisture, it would instantly on its mixture with hydrochloric acid have rendered the medium within the tube turbid. However slight this turbidity might be, and however invisible to the eye, the galvanometer would have revealed it. But there was no movement in an upward direction; the needle gradually sunk from the moment the air entered.

As regards the influence of chemical union in the absorption of radiant heat, a still more severe test than that furnished by the substances last examined is presented by bromine and hydrobromic acid; for the opacity of the former to light is far greater than that of chlorine, while the two compounds are equally transparent. The density of bromine, moreover, is 5.54, whereas that of hydrobromic acid is only 2.75. The difficulty of operating with the acid compound of bromine is at least equal to that attendant on hydrochloric acid; and several successive days were spent in endeavouring to arrive at safe conclusions in connexion with this subject. Bromine dried with phosphoric acid was introduced into a flask furnished with a screw cap, which enabled it to be attached to the experimental tube. By turning a stop-cock, the pure vapour was allowed slowly to enter until the mercury column was depressed two inches. From more than twenty experiments made with this substance, I should infer that the absorption of the quantity mentioned does not exceed 11, while the absorption of hydrobromic acid of the same tension amounts to 30.

The hydrobromic acid was prepared by the action of glacial phosphoric acid (for a pure specimen of which I have to thank my friend Dr. Frankland) on bromide of potassium. If the above figures represent the truth (and I have spared no pains to arrive at a right conclusion), we have here a most striking instance of *transparency to light and opacity to obscure heat being promoted by the self-same chemical act**.

* A layer of liquid bromine, sufficiently opaque to intercept the entire luminous rays of a gas-flame, is highly diathermanous to its obscure

§ 4. In the following Table is given the absorption of a number of gases at a common tension of one atmosphere.

TABLE I.

Name.	Absorption.	Name.	Absorption.
Air	1	Carbonic acid	90
Oxygen	1	Nitrous oxide	355
Nitrogen	1	Sulphuretted hydrogen.	390
Hydrogen	1	Marsh-gas	403
Chlorine	39	Sulphurous acid	710
Hydrochloric acid	62	Olefiant gas	970
Carbonic oxide	90	Ammonia	1195

Air, oxygen, nitrogen, and hydrogen are all set down as equal to unity in the above Table. I do not mean thereby to affirm that there are no differences between these gases as regards their powers of absorption, but that the most powerful and delicate tests which I have hitherto applied have failed to establish a difference in a satisfactory manner. It is not improbable that the action of these gases may turn out to be less even than I have found it. For who can say that the best-constructed drying-apparatus is really perfect? Besides, stopcocks must be greased, and hence may contribute an infinitesimal impurity to the air passing through them. I cannot even say that sulphuric acid, however pure, may not deliver a modicum of vapour to the current of air passing through it. At all events, if any further advance should be made in the purification of the gases, it will certainly only tend to augment the enormous differences exhibited in the above Table.

Ammonia, of the tension mentioned, stands highest in the above list as regards absorptive energy. I believe that a length of less than 3 feet of this gas, which to the vision is as transparent within the tube as the vacuum itself, is *perfectly black* to the rays emanating from the source here made use of. When the gas was in the tube, the interposition of a double metallic screen between the pile and source augmented the deflection very slightly. But I shall show, further on, that the ammonia in this experiment could not exhibit the full energy of its absorption, and that the length indicated is in all probability absolutely impervious to the heat issuing from our source.

It would be a mere affectation of accuracy to try to deal with smaller quantities of the first four substances mentioned in the Table than those with which I have here operated. Still, if

rays. An opaque solution of iodine in bisulphide of carbon behaves similarly. The details of these experiments shall be published in due time: they were publicly shown in my lectures many months ago.—June 13, 1862.

such small quantities could be directly measured, the action of air, oxygen, hydrogen, and nitrogen, in comparison with that of the other substances at the same tension, would doubtless be greatly reduced. With the energetic gases the rays are most copiously struck down by the quantities which first enter the tube, the quantities which enter last producing in many cases an infinitesimal effect. Now I have shown in my last paper that, for very small absorptions, the effect is sensibly proportional to the quantity of gas present; and this would seem to justify us in assuming that for 1 inch of tension the absorption of air, oxygen, nitrogen, and hydrogen would be $\frac{1}{30}$ th of the absorption at 30 inches. With all the other gases I have measured directly the absorption of a quantity possessing in each case a single inch of tension. Assuming the proportionality just referred to, and again calling the effect of air unity (the unit, however, being only $\frac{1}{30}$ th of that in the last Table), the following are the relative absorptions:—

TABLE II.

Air	1	Carbonic oxide	750
Oxygen	1	Nitric oxide	1590
Nitrogen	1	Nitrous oxide	1860
Hydrogen	1	Sulphide of hydrogen .	2100
Chlorine	60	Ammonia	7260
Bromine	160	Olefiant gas	7950
Hydrobromic acid .	1005	Sulphurous acid . . .	8800

Here we have the extraordinary result, that, for tensions of 1 inch of mercury, *the absorption of ammonia is over seven thousand times, the absorption of olefiant gas seven thousand nine hundred and fifty times, while the action of sulphurous acid is eight thousand eight hundred times that of air.*

It is impossible not to be struck by the position of chlorine and bromine in this Table. They are elements, and, notwithstanding their colour and density, they take rank after the transparent elementary gases. The perfectly transparent olefiant gas absorbs more than one hundred and thirty times the amount absorbed by the untransparent chlorine, and nearly fifty times the quantity absorbed by the intensely brown vapour of bromine. I cannot think this fact insignificant. Hitherto chemists have spoken to us of elements, and we have helped ourselves to conceptions regarding them and their compounds in the only way possible to our mental constitution. But our conceptions remained purely subjective, nor were we acquainted with any physical trait which would in any degree justify these conceptions. Here, however, we seem to touch the ultimate particles

of matter. Starting from the idea that a gas absorbs such vibrations as are isochronic with its own, in all cases the compound gas reveals itself to the mind's eye with its molecules on the whole swinging more slowly than the uncombined atoms of which it is composed. Their absorption of the longer undulations proves their general coincidence in period with those undulations. We load the atom by the act of chemical union, and thereby render its vibrations more sluggish, that is to say, more fit to synchronize with the slowly recurrent waves of obscure heat.

In the foregoing Table I have given the absorption of nitric oxide as 1590, which is less than that of nitrous oxide, though the molecule of the former contains a greater number of atoms than that of the latter. It will be noticed *that those gases which on combining suffer no condensation are less energetic absorbers than those which suffer a reduction of volume.* Whether this rule is universal I am as yet unable to say.

It is very difficult to operate with nitric oxide; the affinity of the gas for oxygen is so enormous that the slightest trace of this substance gives rise to the brown fumes of nitrous acid. On first sending this gas into the experimental tube, 1 inch of it gave an absorption of 2040; but the needle slowly went up afterwards, until it finally indicated an absorption of 5100. On looking across the tube at this time, the brown hue of nitrous acid was discernible.

In a second experiment I made the vacuum as perfect as possible; on allowing nitric oxide to enter, the absorption was 1860, but the needle soon afterwards declared an absorption of 3060, the brown fumes appearing as before.

On filling the experimental tube with nitrogen, then exhausting, and allowing nitric oxide to enter, the absorption of 1 inch of the gas was 1680. On filling the experimental tube previously with hydrogen the absorption was 1590, which is that given in the Table. On letting in a mixture of air and nitric oxide till the tube was filled, the action last mentioned was augmented nearly twentyfold. Nitrous acid is therefore an extremely energetic gas. The difference between it and bromine is enormous, when the colours of both are equally dense.

A close inspection of Melloni's Table* reveals, I think, the tendency of solid bodies also to become more transparent to heat as their composition becomes more simple. After rock-salt itself, comes the element sulphur, and after it fluor-spar. But the case of lampblack will here occur to many, who regard it as the most powerful absorber and radiator yet discovered. No doubt the grouping of the atoms of an elementary substance may

* *La Thermochrose*, p. 164.

make it tantamount to a compound, and no doubt this is actually the case with lampblack; another eminent example of this kind is referred to in this paper in the section on ozone. Leslie, however, found water to be a better radiator than lampblack, and Wells found several substances which were more capable of being chilled by nocturnal radiation. On reflection, moreover, the following considerations arise. The lampblack of commerce and the soot of a lamp or candle, that is to say, the substances which have been hitherto employed in experiments on radiant heat, are copiously mixed with hydrocarbons, which are the most powerful absorbers and radiators in Nature. It might fairly be questioned whether the reputed experiments with lampblack really dealt with lampblack at all. But even the impure substance is to some extent transparent to radiant heat.

I have plates of black glass, rendered so by the solution of carbon in the glass while in a state of fusion, and which, though they are impervious to the rays of the most intense electric light, allow of a copious transmission of the rays of obscure heat. Melloni's beautiful experiments on glass of this character are well known; indeed mine are but repetitions of his. Another of Melloni's experiments which I have recently verified is the following. A plate of transparent rock-salt was placed over a smoky camphine lamp, and soot was deposited on its surface until it intercepted every ray of a brilliant jet of gas. The plate was placed between a source of heat possessing a temperature of 100° C. and a thermo-electric pile, a polished screen being placed between the salt and the source. As long as the screen remained, the needle of the galvanometer connected with the pile stood at zero; but the moment the screen was removed the needle promptly advanced, showing the instantaneous transmission across the layer of soot of a portion of the heat incident upon the salt. The actual numbers obtained in this experiment are these:—The deflection produced by the heat transmitted through the soot was 52° ; which is equal to 90 units. The deflection produced when the layer of soot had been carefully removed, so as to leave both surfaces of the salt smooth and transparent, was 71° , which is equal to 300 units. The quantity transmitted through the soot is therefore to the total quantity as

$$90 : 300,$$

or as

$$30 : 100;$$

that is to say, the lampblack, which was perfectly opaque to the light of a gas-jet, was transparent to fully 30 per cent. of the incident heat. On consulting Melloni's Table, I was gratified to find that he made the transmission by a plate similarly pre-

pared 27 per cent.; while a layer so opaque that it cut off the beams of the sun itself, transmitted 23 per cent. of the rays emitted by a source heated to 100° C.

At page 93 of *La Thermochrose*, Melloni examines the absorption of this substance for all sorts of rays, and by a series of ingenious experiments, and reasonings remarkable for their clearness and precision, he arrives at the conclusion that lamp-black absorbs with the same intensity all descriptions of radiant heat*. At page 284, however, he cites and discusses with the same precision a series of experiments made with smoked rock-salt, in which he shows that the same layer of lampblack transmits 8 per cent. of the rays from a lamp of Locatelli, 10 per cent. of those of incandescent platinum, 18 per cent. of those from copper heated to 400° C., and fully 23 per cent. of those emitted by a source of 100° C. Now a transmission of 8 per cent. implies an absorption of 92; while transmissions of 10, 18, and 23 per cent. imply absorptions of 90, 82, and 77. But that the self-same layer of lampblack absorbs 77 per cent. of the rays from one source and 92 per cent. of the rays from another, is at variance with the statement that lampblack absorbs heat from all sources with the same intensity. Suppose the surface of a thermo-electric pile to be coated by a layer of lamp-black of the same thickness as that which coated Melloni's plate of salt; 23 per cent. of the rays from a source of 100° C. would go right through such a layer and impinge upon the metal face of the pile; and if the latter were a good reflector, the heat incident upon it would be in great part retransmitted through the lampblack and lost to the instrument. For a source of 100° C., this loss would be many times greater than for a Locatelli lamp. Possibly, however, Melloni meant simply to assert that for practical purposes the absorption by the face of his pile might be considered to be the same for all kinds of heat†.

§ 5. I have now to record some new experiments on the action of *vapours* upon radiant heat. A number of glass flasks were prepared, of the shape and size of common test-tubes, each of which was furnished with a brass cap carefully cemented on to it. By means of this it could be attached to a stopcock, and thus connected with the experimental tube. The mode of operation was this. The liquid was introduced into the flask by means

* "Donc, le noir de fumée absorbe avec la même intensité toute sorte de rayonnements calorifiques" (p. 101).

† The sun, through the floating carbon of the London atmosphere, sometimes presents a most instructive appearance. Entirely shorn of his rays and of perfectly uniform brightness, his colour at times is as red as blood. This is doubtless mainly due to the comparative transparency of the floating carbon for the longer undulations.

of a small glass funnel; the stopcock (S) was then attached to the flask and connected with a second air-pump, which was always kept at hand. The air above the liquid was removed, and the air dissolved in it was allowed to bubble away, until nothing remained but the pure liquid below and the pure vapour above it. The stopcock S was now shut off, and the flask united to the experimental tube. The exhaustion of the tube and stopcocks being complete, and the needle of the galvanometer at zero, the cock S was turned on and the mercury-gauge carefully observed at the same time. No bubbling of the liquid was in any case permitted. The vapour entered silently and without the slightest commotion; and when the mercurial column was depressed to the extent required, the cock S was promptly turned off.

The energy with which the needle moves the moment a strong vapour enters is so extraordinary, that I was compelled to remove the stops which arrested the swing of the needle at 90° , lest the shock against them should derange the equilibrated magnetism of the astatic pair. The needle often swung far beyond a quadrant; and after it had come finally and permanently to rest, its position was observed in the following manner:—The dial of the galvanometer being well illuminated, a looking-glass was placed behind the instrument, at such an angle that when looked at horizontally the image of the dial was clearly seen. This image was observed by an excellent telescope fixed at a distance of 11 feet from the galvanometer. Attached to the needle, and in continuation of it, was a bit of glass fibre of extreme fineness, which ranged over the graduated circle; and by means of it a very small fraction of a degree could be easily read off. I resorted to the expedient of observing from a distance, because I found that the approach of my person, perhaps through the diamagnetic action of my own body, had a sensible effect upon the needle of my instrument, which, I believe, surpasses in delicacy any hitherto constructed.

The *permanent* deflection of the needle was noted in all these experiments, and the value of the deflection, expressed in terms of one of the lower degrees of the galvanometer, was obtained from a table of calibration. To spare unnecessary labour I omit the deflections in the following Table, and give the absorptions only produced by the vapours mentioned, at 0.1, 0.5, and 1.0 inch of tension.

TABLE III.

Name of substance.	Tensions.		
	0·1 inch.	0·5 inch.	1·0 inch.
Bisulphide of carbon . . .	15	47	62
Iodide of methyle . . .	35	147	242
Benzole . . .	66	182	267
Chloroform . . .	85	182	236
Methylic alcohol . . .	109	390	590
Iodide of ethyle . . .	158	290	390
Amylene . . .	182	535	823
Sulphuric ether . . .	300	710	870
Alcohol . . .	325	622	
Formic ether . . .	480	870	1075
Acetic ether . . .	590	980	1195
Propionate of ethyle . . .	596	970	
Boracic ether . . .	620		

Let us compare some of the results of this Table of transparent vapours with the action of the highly coloured vapour of bromine. The absorption of bromine vapour at 1 inch tension is about 6, and at 0·1 of an inch tension would probably not exceed 1; hence at 0·1 of an inch tension, bisulphide of carbon has probably 15 times the absorbent power of bromine; but bisulphide of carbon is the feeblest of the compound vapours that I have yet discovered. The strongest of these, boracic ether, has, according to the above estimate, and at the tension stated, *more than 600 times the absorbing energy of the strongly coloured bromine.*

The whole of the numbers in the above Table are referred to atmospheric air as unity; 0·1 of an inch of bisulphide-of-carbon vapour, for example, absorbs 15 times as much as a whole atmosphere of air. Let us compare, for an instant, the action of boracic ether with that of air. We arrive at an approximate comparison in this way. The absorption of the tenth of an inch of boracic ether is something more than that of a whole inch of methylic alcohol; by diminishing the quantity of methylic alcohol to one-tenth, we reduce its absorption from 590 to 109. The absorption of one-tenth of an inch of boracic ether is 620°; suppose it to diminish in the proportion above found for methylic alcohol, we should have for 0·01 of an inch of boracic ether an absorption of 111; that is to say, for $\frac{1}{3000}$ th of an atmosphere of boracic ether, we should have an absorption 111 times that of a whole atmosphere of oxygen, nitrogen, and hydrogen, or atmospheric air.

With the transparent elementary gases it is impossible to measure directly the absorption of 0·1 of an inch; but assuming,

as before, that up to an absorption of 1 the effect is proportional to the quantity of gas present, the absorption of each of the elementary gases, at a tension of 0.1 of an inch, would be about 0.0033; hence the absorption of boracic ether of 0.1 of an inch tension is to that of air at the same tension as

$$620 : 0.0033,$$

which would give to the ether an energy 186,000 times that of air.

I have already spoken of the blackness of ammonia at 30 inches tension. Referring to Table I., its absorption is found to be 1195. In the last Table the vapour of acetic ether, possessing only one-thirtieth of the tension of the ammonia, produces apparently the same effect; its absorption is also 1195. Such facts give one entirely new ideas of the capabilities of matter; and our wonder will not be diminished by the results to be recorded further on.

With both gases and vapours we find that it does not follow that a gas which produces a larger effect than another at one tension should surpass that other at all tensions. Some gases start from a lower level than others, but finally attain an equal, or even a greater elevation. If their absorptions were represented by curves plotted from the same datum-line, these curves would in some cases approach, and in some cases cross each other. At a tension of 1 inch, for example, carbonic acid has more than double the absorptive power of carbonic oxide, whereas at a tension of 30 inches they are equal; indeed some of my experiments show carbonic oxide to have the advantage. On the 22nd of October, for example, I found the deflection produced by 2 inches of carbonic oxide to be 15° , while that of 2 inches of carbonic acid was 38° . The two gases at a tension of 30 inches gave these results:—

Carbonic oxide	52°
Carbonic acid	51.5

And again, on the 4th of November I obtained the following relative effects:—

	Tensions.	
	1.2 inch.	24 inches.
Carbonic oxide	12°	57°
Carbonic acid	37	54

The same remarks apply to vapours. Methylic alcohol, for example, starts at a lower level than the iodide of ethyle, but ascends more quickly, and finally reaches a much higher elevation. The same observation may be made of chloroform in comparison with benzole and the iodide of ethyle.

[To be continued.]

XXXVII. *On Symbolical Decomposition; and on the last two Papers of Mr. Jerrard.* By JAMES COCKLE, M.A., F.R.A.S., F.C.P.S. &c.*

1. **T**HE identical equality

$$\frac{dy}{dx} - \frac{dy}{dx} = 0$$

may be written thus,

$$\frac{dy}{dx} - \frac{1}{y} \cdot \frac{dy}{dx} \cdot y = 0;$$

or, symbolically,

$$\left(\frac{d}{dx} - \frac{1}{y} \cdot \frac{dy}{dx} \right) y = 0;$$

consequently

$$y = \alpha$$

is a solution of

$$\left(\frac{d}{dx} - \frac{1}{\alpha} \cdot \frac{d\alpha}{dx} \right) y = 0,$$

and therefore of

$$\left(\frac{d}{dx} - \beta \right) \left(\frac{d}{dx} - \frac{1}{\alpha} \cdot \frac{d\alpha}{dx} \right) y = 0,$$

the symbolical development of which is

$$\left\{ \frac{d^2}{dx^2} - \left(\beta + \frac{1}{\alpha} \cdot \frac{d\alpha}{dx} \right) \frac{d}{dx} + \frac{\beta}{\alpha} \cdot \frac{d\alpha}{dx} + \frac{1}{\alpha^2} \left(\frac{d\alpha}{dx} \right)^2 - \frac{1}{\alpha} \cdot \frac{d^2\alpha}{dx^2} \right\} y = 0. (1)$$

And if (1) be identical with

$$\left(\frac{d^2}{dx^2} + 2q \frac{d}{dx} + r \right) y = 0, \quad . \quad . \quad . \quad . \quad . \quad (2)$$

we have

$$-\beta = 2q + \frac{1}{\alpha} \cdot \frac{d\alpha}{dx}, \quad . \quad . \quad . \quad . \quad . \quad (3)$$

$$r = \frac{\beta}{\alpha} \cdot \frac{d\alpha}{dx} + \frac{1}{\alpha^2} \left(\frac{d\alpha}{dx} \right)^2 - \frac{1}{\alpha} \cdot \frac{d^2\alpha}{dx^2}. \quad . \quad . \quad . \quad (4)$$

Now α , a particular integral of (2), being known, β is known by means of (3), and the symbolical decomposition of (2) is effected. On my communication to Mr. Harley of the foregoing solution of the problem of decomposition, with a suggestion of a possible difficulty, that mathematician, with his usual promptitude, pointed out that (3) and (4) are not independent conditions,

* Communicated by the Author.

and that on eliminating $\frac{1}{\alpha} \cdot \frac{d^2\alpha}{dx^2}$ from (4) by means of the relation

$$\frac{1}{\alpha} \cdot \frac{d^2\alpha}{dx^2} = -\frac{2q}{\alpha} \frac{d\alpha}{dx} - r,$$

which, since α is supposed to be a particular integral of (2), is seen to be satisfied, we are led to a result substantially identical with (3). I may add that, if (2) and

$$\left(\frac{d^2}{dx^2} + 2q \frac{d}{dx} + s\right)y = 0 \quad . \quad . \quad . \quad . \quad . \quad (5)$$

have a common solution, α , then, α not vanishing,

$$s = r$$

necessarily.

I have applied the same process to a linear differential equation of the third order. Its application, however, seems to require an artifice needless in the decomposition of equations of the second order.

2. If it be thought needful to put M. Hermite's final result under the form indicated in art. 10 of Mr. Jerrard's "Supplementary Remarks," &c., it must also be borne in mind that N is a function of roots of the *réduite*, and that the case put by Mr. Jerrard in art. 11 is (in Mr. Jerrard's point of view) explained by the fact that in such case the irreducible quintic radicals are contained in the roots of the *réduite*. However, although Mr. Jerrard has conceded, I think more than once, that the roots of a general quintic cannot be expressed without quintic surds, yet he is no doubt entitled to adopt, for the purposes of arts. 12 and 13, the hypothesis that the roots can be expressed in terms of cubic and quadratic radicals only. But why is this hypothesis shifted in art. 14? The hypothesis being supposed true, Mr. Jerrard, in art. 13, concludes that \mathfrak{D} ought to be different from zero. The hypothesis being supposed false, Mr. Jerrard, in art. 14, is forced to come to the conclusion that \mathfrak{D} ought to be equal to zero. The antecedents of these hypothetical propositions are contradictory; and though we may, by combining them, arrive at the result that, if the hypothesis be supposed to be simultaneously true and false, \mathfrak{D} ought to be simultaneously different from and equal to zero, yet such a result is nugatory, and does not establish any incongruity or error whatever in M. Hermite's argument. Mr. Jerrard's argument being invalid, it is unnecessary to inquire whether he is justified in assuming that the denominator of an expression indicating an impossibility is zero.

3. I would rather be regarded as a student than a critic of

Mr. Jerrard's writings; but with reference to arts. 7 and 8 of his last "Note," &c., I must say that the restrictions which he seeks to impose on Lagrange's theory of similar functions seem to me unwarrantable. Who would think of superadding a second rational relation, as suggested in art. 8? If the value of v given in that article coincides with the value given by (e_1) , then (e_1) may be expunged as superfluous; if the values do not coincide, then one of the repugnant equations has been improperly introduced. In employing Lagrange's theory we may confine our attention to (E_1) , and need not concern ourselves about (e_1) in any way.

Felixstowe, by Ipswich, Suffolk,
September 2, 1862.

XXXVIII. *Final Remarks on Mr. Jerrard's Theory of Equations of the Fifth Order.* By A. CAYLEY, Esq.*

MR. JERRARD, in his paper, "Note on some Objections of Mr. Cayley and Mr. Cockle," in the September Number, p. 193, concludes, so far as relates to me, as follows:—"The curious irrelevancy of Mr. Cayley's objection will now be seen. He merely occupies himself in proving what I had taken for granted, while he leaves untouched the main question to be discussed, which is this:—Is Lagrange's theory inapplicable to the case of $u=\alpha$, $v=\alpha^5$, in virtue of the equation (e'_1) ?" But if my objection be (curiously or otherwise) *irrelevant*, then the proposition I contend for might be admitted without prejudice to Mr. Jerrard's results: this proposition is, that Lagrange's theory is applicable to the case of $u=\alpha$, $v=\alpha^5$, which is a case not excluded by the only exception (the case of equal roots) to the general theory, and therefore, notwithstanding the equation (e'_1) or anything else whatever, coming within the general theory. Mr. Jerrard, in his reply, contends for the contradictory proposition that, in virtue of the equation (e'_1) , Lagrange's theory is not applicable to the case $u=\alpha$, $v=\alpha^5$, and he thus in effect treats the objection as a *relevant* one. It appears to me that the objection is not only a relevant one, but that the proposition therein contended for is completely proved; at any rate the issue is so narrow a one that it seems useless to argue it further, and it is not my intention to do so.

2 Stone Buildings, W.C.,
September 13, 1862.

* Communicated by the Author.

XXXIX. *Continuation of the Hydrodynamical Theory of Vibrations.* By Professor CHALLIS, M.A., F.R.S., F.R.A.S.*

32. **B**EFORE proceeding to the physical explanations which are the main object of this hydrodynamical theory, it will be necessary to enter upon the discussion of certain points which were omitted, or only alluded to, in the communication to the August Number. One point in particular, mentioned in art. 18, is the determination of the motion for the case in which the directions of motion are, by the conditions of the disturbance, perpendicular to a given plane. Let the motion be originally produced by a plane surface of indefinite extent, always parallel to the given plane, and oscillating in a given manner. It is evident that under these circumstances, however large the oscillations may be, the motion is wholly in directions at right angles to the plane. As it does not appear to be possible to give an exact solution of this problem on the principles about to be indicated, an approximative method will be adopted, and the reasoning will generally be restricted to terms of the first order. The results thus obtained are sufficiently approximate for the applications which will be made of them in explaining phenomena of light, while at the same time they are necessary steps towards a nearer approximation, if for other purposes it should be required to take account of terms of a higher order.

33. The exact equation (5) in art. 21, applicable to motion along a rectilinear axis of free motion, becomes, after excluding terms of a higher order than the first, the linear equation (6), viz.

$$\frac{d^2\phi}{dt^2} - a^2 \frac{d^2\phi}{dz^2} + b^2\phi = 0;$$

from which was deduced (arts. 22 and 30) the particular solution

$$\phi = m \cos \frac{2\pi}{\lambda} (z - \kappa at + c),$$

κ being put for the numerical quantity $\left(1 + \frac{4}{\pi}\right)^{\frac{1}{2}}$. Since the above linear equation has constant coefficients, the solution expressed in all its generality is

$$\phi = \Sigma . m \cos \frac{2\pi}{\lambda} (z - \kappa at + c),$$

an unlimited number of terms being included under the sign Σ , having different values of the constants m , λ , and c . This equation gives, for the general value of the velocity (V) along the axis,

$$V = \frac{d\phi}{dz} = \Sigma . \mu \sin \frac{2\pi}{\lambda} (z - \kappa at + c),$$

* Communicated by the Author.

μ being put in the place of $-\frac{2\pi m}{\lambda}$. Also since to the approximation of the first order the condensation (σ) on the axis is $-\frac{1}{a^2} \cdot \frac{d\phi}{dt}$, we have

$$\sigma = \frac{\kappa}{a} \cdot \Sigma \cdot \mu \sin \frac{2\pi}{\lambda} (z - \kappa at + c).$$

Hence

$$V = \frac{a}{\kappa} \cdot \sigma.$$

This is the general relation between the velocity and density at any points of the axis for waves propagated in the positive direction of z . The relation is the same between the velocity parallel to the axis and the condensation at points not on the axis, the velocity at such points being Vf , and the condensation σf . These velocities coexist with transverse velocities expressed by $\phi \frac{df}{dx}$ and $\phi \frac{df}{dy}$.

34. Hitherto the reasoning has had reference to the motion relative to a *single* axis of free motion. An important remark is now to be made previous to taking the next step towards the solution of the problem of rectilinear motion perpendicular to a plane. Since the law of longitudinal and transverse vibration relative to an axis was demonstrated generally, and prior to any supposed case of disturbance, it must receive application, whatever be the disturbance. Hence, as a necessary consequence, any motion arbitrarily impressed on the fluid must be regarded as *composed* of motions subject to that law. This principle being admitted, and the motion in the instance before us being wholly parallel to a given direction, it follows that the component motions must be so arranged that the transverse motions destroy each other. This will be the case if the axes be all parallel to the given direction and be unlimited in number, and if the phases of vibration be the same for all. For, the disturbing plane being assumed to be of unlimited extent, there will thus be no reason for transverse motion in one direction rather than another, and there will consequently be no transverse motion. Now, let the disturbance of the fluid be conceived to be such that the velocity at the given distance z_1 from the origin is the arbitrary quantity $F(\kappa at)$ at the time t . Since it may be assumed, in accordance with a recognized analytical principle, that the successive values of this function at the given position can be expressed as nearly as we please by $\Sigma \cdot \mu \sin \frac{2\pi}{\lambda} (z_1 - \kappa at + c)$, the number of the terms under the sign Σ , and the values of μ , λ , and c for each

term being arbitrary, it follows from the preceding reasoning that these values of $F(\kappa at)$ are propagated with the velocity κa , and that for any distance z from the origin we have

$$V = F(\kappa at - z + z_1).$$

It is here to be remarked that there is no reason from the foregoing argument to conclude that this value of V should satisfy the equation (6) when any arbitrary form is given to the function F , inasmuch as that equation applies only to motion along a *single* axis, accompanied by transverse motion.

35. We have now to find the relation between the velocity and condensation in the case under consideration, of motion in parallel straight lines. The relation cannot be the same as that obtained in art. 33 for motion along an axis, because the condensation on the axis must be affected as well by the transverse as by the longitudinal vibrations. Since, from what is shown above, the rate of propagation of the velocity is constant, it may be proved by reasoning given under Prop. XII. in the Number of the Philosophical Magazine for February 1853, that a necessary relation exists between the velocity and the corresponding density. For the sake of distinctness I shall here repeat so much of that reasoning as is required for completing the present argument. Let V and ρ be the velocity and density of the fluid which passes the transverse section m of a slender straight tube at the distance z from the origin at the time t , and let V' and ρ' be the velocity and density of the fluid which at the same instant passes the transverse section m' situated in advance of the other by the interval δz . The increment of fluid between the two

sections in the interval from $t - \frac{\delta t}{2}$ to $t + \frac{\delta t}{2}$ is $m\rho V\delta t - m'\rho'V'\delta t$,

because the changes of ρV and $\rho'V'$ in the small interval δt may be supposed to be proportional to that interval, and may therefore be omitted. Let the above increment become equal to the excess

of the quantity of fluid at the time $t - \frac{\delta t}{2}$ in the element of length δz terminating at the section m , above the quantity of fluid at the same time in the element between the sections m and m' . That is, neglecting terms involving $\delta z\delta t$ and δz^2 , let

$$m\rho V\delta t - m'\rho'V'\delta t = m\rho\delta z - m'\rho'\delta z.$$

Then the mean density of the former element will have been transferred through the space δz in the time δt ; and $\frac{\delta z}{\delta t}$ is its rate of propagation, which ultimately applies to the density ρ . Hence by the above equation, passing from differences to differentials,

$$\frac{d \cdot m\rho V}{dz} = \frac{d \cdot m\rho}{dz} \cdot \frac{\delta z}{\delta t}.$$

Now let $\frac{\delta z}{\delta t} =$ the constant a' . Then by integrating,

$$m\rho V = a'm\rho + C;$$

and if C be determined so that $V=0$ where $\rho=1$, we have

$$\rho V = a'(\rho - 1); \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

from which equation, it may be noticed, the variable m has disappeared. This result gives the relation between the velocity and density, on the supposition that the *density* is propagated uniformly; and from it we may infer that *both* must be propagated uniformly at the same time. To the first order of approximation, $V = a'\sigma$. Applying this relation in the problem before us, it having been shown that V is propagated with the uniform velocity κa , we have

$$V = \kappa a \sigma = F(\kappa at - z + c).$$

36. If the propagation had taken place in the opposite direction, we should have similarly obtained

$$V' = -\kappa a \sigma' = f(\kappa at + z + c').$$

As the equations from which these results are deduced are linear with constant coefficients, motions propagated in opposite directions may coexist. Hence putting V_1 for $V + V'$, and σ_1 for $\sigma + \sigma'$, we shall have generally,

$$V_1 = F(\kappa at - z + c) + f(\kappa at + z + c'),$$

$$\sigma_1 = F(\kappa at - z + c) - f(\kappa at + z + c').$$

From these equations we obtain by differentiation,

$$\kappa^2 a^2 \cdot \frac{d\sigma_1}{dz} + \frac{dV_1}{dt} = 0. \quad . \quad . \quad . \quad . \quad . \quad (15)$$

This equation is applicable generally to motion in parallel straight lines. Now when the motion is such, a prismatic portion of the fluid of indefinitely small transverse section may be conceived to be separated from the rest, and to be contained within rigid boundaries. The above equation will thus apply to *constrained* rectilinear motion; and it may be remarked that it differs from the analogous equation applicable to free motion by having the factor κ^2 in the first term. It should also be noticed that the composite character of the motion is a physical reality in constrained rectilinear motion.

37. Although in the investigation of equation (15) the motion was supposed to be in parallel straight lines, that equation is equally applicable to motion within slender tubes of variable transverse section, if the inclinations of the surface to the axis be everywhere indefinitely small. For the factor κ^2 simply takes account of the effect of preventing transverse motion; and as

this is equally prevented whether the infinitely small transverse sections be uniform, or variable in the manner just mentioned, the same equation applies in all cases. Let us suppose, for instance, that the disturbance of the fluid is such that the motion is constrained to take place in all directions from a centre, and so as to be a function of the distance from the centre. It will be sufficient in this case to consider the motion in a slender pyramidal tube bounded by planes passing through the centre; and if V and σ be the velocity and condensation at the distance r from the centre, we have by the equation above,

$$\kappa^2 a^2 \cdot \frac{d\sigma}{dr} + \frac{dV}{dt} = 0.$$

Also by the equation of constancy of mass,

$$\frac{d\sigma}{dt} + \frac{dV}{dr} + \frac{2V}{r} = 0.$$

These two equations conduct to the usual solution of the problem of central motion, with the exception that κa takes the place of a . It must, however, be borne in mind that the foregoing investigation rests on the law of composite motion, the components being, as in the case of motion perpendicular to a plane, vibrations expressed by circular functions. By means of this composition it is possible to explain how the condensation varies inversely as the distance, and not inversely as the square of the distance, from the centre. It does not fall within my present purpose to discuss at greater length the case of central motion.

38. Not only in the cases above considered of parallel and central rectilinear motions, but in every instance of disturbance of the fluid, however produced, both initially and subsequently the velocity and condensation must be regarded as compounded of velocities and condensations defined by the functions ϕ and f . This follows from the circumstance that the forms and values of these functions were determined antecedently to the consideration of any case of disturbance. To satisfy the conditions of a given disturbance, the number and directions of the axes of rectilinear motion, and the values of m , λ , and c , are at disposal.

39. According to the determinations that have been made of the forms of ϕ and f , the vibrations which take place relative to a single axis of rectilinear motion, which, as being independent of the mode of disturbance, I have called for distinction *free* motions, are such as follow. The motion along and parallel to the axis is represented to the first approximation by

$$mf \sin \frac{2\pi}{\lambda} (\kappa at - z + c);$$

and if this quantity be called $f \frac{d\phi}{dz}$, the motion transverse to the axis is represented by $\phi \frac{df}{dr}$, r being any distance from the axis. It

is particularly to be observed that, by the demonstration in art. 28 (Phil. Mag. for August), the free motion is exclusively that which is symmetrical about the axis, and the condensation and transverse velocity are consequently functions of r . The condensation at any point of the axis at any time being $-\frac{d\phi}{a^2 dt}$,

that at any position in the same transverse plane at the same time is $-\frac{f}{a^2} \cdot \frac{d\phi}{dt}$. From these expressions, and by taking

account of the properties of the function f , it will be seen that there are positions of maximum condensation, positive or negative, and of no condensation, and intermediate positions of maximum transverse velocity, positive or negative, and of no transverse velocity, in fixed cylindrical surfaces about the axis, and that the number of such surfaces is unlimited. The maxima both of condensation and of transverse velocity continually decrease with the distance from the axis; and the intervals between their consecutive positions, as also the intervals between the consecutive positions of no condensation and no transverse velocity, go on decreasing till they reach a certain limiting value, which by the reasoning in art. 30 was ascertained to be $\frac{1}{\sqrt{e}}$. Now,

although this be the character of free motion, it by no means follows that the motion relative to an axis is necessarily such. In fact, the partial differential equations (10) and (11), by which the values of f and σ are determined, prove that the motion may be subject to arbitrary conditions. But as the motion in every instance exists originally in the form of free motion, the arbitrary motion is subject to the limitation of being such only as may result from modifications of that motion. For instance, conceive that the free motion relative to a given axis is by some disturbance separated into two parts; which is an allowable supposition, because the equations determining the motion are linear with constant coefficients. The parts may or may not subsequently follow the same course. The resolution must take place in such a manner that the sum of the condensations and the sum of the longitudinal motions, at corresponding points of the two parts, must be equal respectively to the condensation and longitudinal motion at the corresponding point of the original motion; and the sums of the resolved parts parallel to the axes of x and y of the two transverse motions, must be equal respect-

ively to the resolved parts of the original transverse motion in the same directions. That is, if the condensation σ at any point of the axis be separated into σ_1 and σ_2 , and if f, f_1, f_2 refer respectively to the original and the bifurcated motions, we shall have the four equations

$$\begin{aligned}\sigma &= \sigma_1 + \sigma_2, & \sigma f &= \sigma_1 f_1 + \sigma_2 f_2, \\ \sigma_1 \frac{df_1}{dx} + \sigma_2 \frac{df_2}{dx} &= \sigma \frac{df}{dr} \cdot \frac{x}{r}, & \sigma_1 \frac{df_1}{dy} + \sigma_2 \frac{df_2}{dy} &= \sigma \frac{df}{dr} \cdot \frac{y}{r};\end{aligned}$$

by which $\sigma_1, \sigma_2, f_1, f_2$ are determined.

40. At the end of the communication of December 1852, I have obtained a particular solution of the equation

$$\frac{d^2 f}{dx^2} + \frac{d^2 f}{dy^2} + 4ef = 0$$

by a process analogous to that which was applied to the equation (6); and as the particular solution of the latter indicated laws of the motion independent of arbitrary disturbances, that of the other must receive a like interpretation. The solution in question is

$$f = \cos \{2 \sqrt{e}(x \cos \theta + y \sin \theta)\},$$

the value of f being supposed to be unity where $x=0$ and $y=0$ and θ being an arbitrary constant. All that can be generally inferred from this value of f is, that independently of the particular mode of disturbance, the motion may be parallel to an arbitrary direction. Let us, therefore, now suppose that the original motion is separated into two motions in fixed directions corresponding to the angles θ_1 and θ_2 . Then, by the reasoning of the preceding article,

$$\begin{aligned}\sigma f &= \sigma_1 f_1 + \sigma_2 f_2 = \sigma_1 \cos \{2 \sqrt{e}(x \cos \theta_1 + y \sin \theta_1)\} \\ &\quad + \sigma_2 \cos \{2 \sqrt{e}(x \cos \theta_2 + y \sin \theta_2)\},\end{aligned}$$

which value of f evidently satisfies the differential equation. By expanding to the second powers of x and y , we have

$$\begin{aligned}\sigma f &= \sigma_1 + \sigma_2 - 2\sigma_1 e(x^2 \cos^2 \theta_1 + 2xy \sin \theta_1 \cos \theta_1 + y^2 \sin^2 \theta_1) \\ &\quad - 2\sigma_2 e(x^2 \cos^2 \theta_2 + 2xy \sin \theta_2 \cos \theta_2 + y^2 \sin^2 \theta_2).\end{aligned}$$

But from the considerations adduced above, this value of σf does not apply to any actual motion unless it be identical with that for free motion, which, to the same degree of approximation, is $\sigma(1 - er^2)$. This condition involves the following consequences:

$$\theta_2 = \frac{\pi}{2} + \theta_1, \quad \sigma_1 + \sigma_2 = \sigma, \quad \sigma_1 = \sigma_2 = \frac{\sigma}{2};$$

that is, the two component motions are at right angles to each other, and of equal magnitude.

41. From the above results it may be concluded that, if the disturbance be such as to separate the original condensation into two unequal parts, there will be equal transverse motions in rectangular directions corresponding to the less, and a portion of the larger equal to the less, while the other portion will remain symmetrically disposed about the axis, just as in the case of free motion.

42. The identity of the values of σf for the original motion and the components is found to extend only to the squares of x and y . Hence this resolution into rectangular motions in fixed directions applies only to motion at distances from the axis so small that the equation $f=1-er^2$ gives the value of f with sufficient approximation. Hence er^2 , or $\frac{4r^2}{\lambda^2}$, must be a very small fraction, and consequently r very small, compared to $\frac{\lambda}{2}$.

Each of these components might also, under certain circumstances, be resolved into two parts. The laws of this second resolution I have treated of in a paper "On the Theory of the Polarization of Light," in the Cambridge Philosophical Transactions, vol. viii. part 3. The mathematical reasoning is too long for insertion here.

43. In art. 34 it is supposed that the oscillating plane which produces the motion is of unlimited extent, in order to avoid the consideration of the lateral motion which would take place near the borders if the plane were finite. Let us now suppose that the parallel axes of the component motions are included within a limited space, for instance a cylinder of given radius, and endeavour to ascertain the kind of motion which prevails at and near this boundary. It is evident that, since at these parts the transverse motion is only partially destroyed, the motion must there be compounded of transverse and longitudinal vibrations. This motion, however, as the following considerations will show, does not spread laterally to an indefinite extent, but is always confined within certain limits. Having regard to the application proposed to be made of these hydrodynamical researches, let us conceive the values of λ to be very minute, and the velocities of the fluid particles to be extremely small compared to the velocity of the propagation of the waves. Then taking account of the characteristics of the component vibrations described in art. 39, it will be seen that, although the vibrations relative to each axis individually are not limited as to distance from the axis, *a limit to the compound motion is imposed laterally by the composition of the vibrations*. Admitting that the number of the axes of the components within a given space may be as large as we please, since the vibrations are by hypothesis

(art. 34) all in the same phase, it will follow that beyond a certain finite distance from the cylindrical surface the sum of the positive condensations may be as nearly as we please equal to the sum of the negative condensations. Thus the resultant condensation will vanish, and there will be neither transverse nor longitudinal motion. As that distance must be many multiples of λ , it can be a small quantity only in case λ be extremely small. The magnitudes of the longitudinal vibrations increase from zero at the limiting distance till they acquire a maximum and uniform value at a certain distance within the cylindrical surface; and the transverse vibrations, increasing from zero at the exterior limit till they reach a maximum near the cylindrical surface, afterwards diminish till they disappear at the interior limit by the counteraction of opposite vibrations. The thickness of the cylindrical shell which, within its interior and exterior surfaces, includes the whole of the transverse motion, will be less as the breadth of the waves is less; and if the waves be of extremely small breadth, it is conceivable that that space and the interior cylindrical space occupied by the motion which is exclusively longitudinal, may together only form a cylinder of very small radius. Thus motion included within such a cylinder would be propagated to an unlimited distance without lateral divergence.

44. It might be objected that, although this reasoning may hold good so long as the component motions are such that the condensations are symmetrically arranged about the axes, it does not apply to components which have undergone the modifications described in articles 40 and 41. To this objection it may be replied that the resolutions of the motion and condensation there considered are confined, as is shown in art. 42, to distances from the axis very small compared to λ , whereas the transverse motion extends to distances which are many multiples of λ . When symmetrical motion and condensation about an axis is separated by any cause into two parts, each part will have positions of no condensation at distances from the axis the same as those at which the condensation was zero previous to the separation. There is, in fact, no reason for concluding that, at positions not extremely close to the axis, the separation produces any other effect than a partition of condensation between the two parts, the directions of the motion remaining the same. Thus the asymmetry near the axes, within very small distances from them, merges into symmetry. This remark may suffice to meet the above-mentioned objection.

45. There remains for consideration another point, viz. the determination of the laws of the propagation of undulations. As these laws are not arbitrary, they must admit of being ascertained independently of the supposition of any case of arbitrary

disturbance. This, in fact, is done by means of the equation (5) in art. 21, which applies accurately to the motion along an axis of free motion. From the particular solution of this equation obtained in articles 22 and 23, it may be inferred that, however far the approximation be carried, and consequently whatever be the magnitude of the motion along the axis, the rate of propagation of the velocity and condensation is absolutely uniform, and such that no change of the type of the undulations is produced by the transmission.

46. But it was found in articles 33 and 34 that the equation (5) might be satisfied to the first approximation by supposing ϕ to be equal to the sum of an unlimited number of terms such as $m \sin q (z - a_1 t + c)$, and that, in order to satisfy the conditions of any given disturbance, it was necessary to express ϕ in this manner; which analytical circumstance indicates that the motion is of a composite character independently of the mode of disturbance. From this first approximation we might proceed to the next by assuming that

$$\phi = \Sigma . \{ m \sin q (z - a_1 t + c) \} + \psi,$$

and substituting this value in (5), omitting the last term of the equation. It would then appear that the equation might be satisfied by supposing ψ to consist of a series of sines of circular arcs with constant coefficients of the second order with respect to m . Also if the values of λ in the series of the first order be restricted within limits not far apart (as is the case in the Theory of Light), those in the series of the next order will be either considerably greater or considerably less. The same thing will happen if the approximation be carried still further. On this account, as well as because ψ is a quantity of a superior order, it is allowable to omit it in the theoretical explanations of phenomena of light. With respect to the terms of the first order, it may be remarked that the considerations in art. 45 seem to justify the inference that each of the components which they represent is propagated with a velocity absolutely uniform; but as the expression for that velocity involves m (art. 24), it must be admitted (unless m be the same for all) that the different components are propagated with somewhat different uniform velocities.

47. I have already adverted (art. 17) to the equations deduced from the received principles of hydrodynamics for the case of motion in straight lines perpendicular to a plane and propagated in the positive direction, viz.

$$w = a \text{ Nap. log } \rho = f(z - (a + w)t),$$

from which it may be inferred that points of no velocity and condensation may at the same time be points of maximum velocity and condensation. The interpretation put upon these equations

by Mr. Airy (Phil. Mag. for June 1849, p. 401) is, that a given wave continually changes type till it is eventually broken up, so that musical sounds are by mere propagation through space converted into unmusical noise. The interpretation of Professor Stokes (Phil. Mag. for November 1848, p. 349) is not essentially different from this. It is not asserted that this interpretation is in accordance with observed facts. Moreover, there lies against it the obvious objection that the very same equations which indicate that musical sounds become unmusical, equally indicate that the latter again become musical, and so on alternately. This is so great an absurdity, that, as there is no reason to question the accuracy either of the principles on which the above equations rest, or of the mathematical process by which they were obtained, the conclusion is inevitable that those principles are insufficient. I have supplied the defect by the discovery of a third fundamental equation, by means of which it has been proved, in this communication, that, in conformity with experience, undulations are propagated through space uniformly and without undergoing change.

48. Having now gone through a revision of my hydrodynamical theorems, so far, at least, as they relate to the approximation of the first order, I have only to add a few general remarks. All the foregoing investigations have had reference to the solution of a problem which may be thus enunciated:—To determine the motions of a continuous mass of unconnected particles subject to given conditions. The investigations have had nothing in them speculative or conventional. The premises are matters of fact on which all mathematicians are agreed; and the required results are to be obtained by reasoning from right principles according to logical rules. If two mathematicians differ in any conclusion, it is because there has been erroneous reasoning on one side, or on both. In a question to be answered by pure reasoning, unanimity ought to be attainable. The problem is different from that of determining the motion of a continuous mass of rigidly connected particles subject to given conditions, which can always be effected by means of ordinary differential equations, being, in fact, virtually the determination of the motion of a single material particle acted upon by given forces. The other problem demands the formation of differential equations of a more comprehensive kind, containing at least three variables; and it is in the application of such equations that the difficulty of solving it lies. Newton discovered the principles and calculations proper for determining the motion of a single particle acted upon by given forces; the discovery of the principles and calculations required for determining the simultaneous movements of the unconnected particles of a continuous mass

can hardly yet be said to be completely effected. The step which I claim to have taken in this direction in the discovery of a third hydrodynamical equation, and the results which have been obtained by the combination of this equation with the other two, deserve, I think, the attention of mathematicians. If any objections should be made to either the principles or the reasoning which I have advanced, I shall be ready to take them into consideration; if any explanations should be asked for, I hope to be able to give them. The subject is one that cannot possibly be ignored if progress in theoretical physics be of any moment. The importance in that respect of the foregoing hydrodynamical theory of vibrations is evinced by the facility with which it at once accounts for the more obvious phenomena of light. This application of it I reserve for another communication, the present one having already extended to a great length.

Cambridge, September 13, 1862.

XL. On the Nature of those Red Protuberances which are seen on the Sun's Limb during a Total Eclipse. By B. STEWART, M.A., F.R.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

WILL you permit me to make a few observations on those very remarkable protuberances which are seen to surround the sun's disk during the time of his total eclipse? and if I am tempted to indulge in speculation, the rare occurrence of the phenomenon, and the interest attaching to it, may perhaps be viewed as an apology.

These strange and startling appearances have now been observed at least three times. The first of these occasions was during the total eclipse of the 7th of July, 1842, when their existence was revealed by the late Mr. Baily and the Astronomer Royal. The account of these philosophers was sufficient to rouse the attention of astronomers; so that during the next total eclipse, which happened on July 28th, 1851, there was assembled a large body of skilful and accurate observers well qualified to make the most of the few but precious minutes of darkness allotted them, in order if possible to elucidate the nature of these striking appearances.

From the observations of Mr. Airy and others, it was rendered very probable that the red protuberances really belonged to the sun, while, from their vast magnitude, the idea of their being solar mountains was at once discarded.

It seemed rather to be the impression among those assembled on this occasion, that they were connected in some way with

solar spots, some of which appeared on the sun's disk in places not far removed from those occupied by the red flames; and Mr. H. F. Talbot suggested that the latter might be heated vapours, which, rushing through the spots or openings in the sun's photosphere, ascend to the upper regions of the solar atmosphere in flames of immense elevation.

The third and most recent occasion on which these protuberances were observed, was during the total solar eclipse of July 18th, 1860, which was attentively watched in Spain by eminent men from all quarters. It was on this occasion that Mr. De la Rue was enabled, by means of the Kew heliograph, to obtain a photographic representation of them; and from the nature of his observations, as well as from the very great care and labour which he has bestowed upon their reduction, no doubt can be entertained of the accuracy of his conclusions.

By his labours, and by those of Mr. Airy and other observers, it is rendered certain that these prominences belong to the sun, and that they are not optical illusions, but represent actual changes of unknown nature but of stupendous magnitude taking place in our luminary, and extending in some instances to a distance of 70,000 miles or more above the photosphere of the sun.

It was also proved on this occasion that the light from the protuberances was unpolarized, and that it had very great actinic power. Many members also of the expedition supposed these appearances to be clouds in the solar atmosphere, to which idea they may have been led by the circumstance that at least one mass appeared to be floating quite detached, 14,000 miles from the body of the sun. Mr. De la Rue also seemed to think that one of the most remarkable of the protuberances was connected in position with a mass of faculæ which came round upon the sun's disk a day or two afterwards.

These are the three occasions on which these phenomena have been observed; and it has been the nearly unanimous belief of all observers who have acknowledged their connexion with the sun, that they represent great disturbances taking place in those regions of the solar atmosphere in which they manifest themselves.

Since, therefore, these are phenomena undoubtedly associated with our luminary, since also they extend to a distance of at least 70,000 miles above the sun's photosphere, and since we cannot well conceive them to exist without the presence of an atmosphere of some sort, however attenuated, we are led to the startling conclusion that the sun's atmosphere extends to at least this distance above its surface.

This conclusion certainly excites a feeling of wonder in the mind, which admits so strange a result with reluctance; and this has induced the writer to seek for an explanation of these ap-

pearances consistent with the presence of a very attenuated atmosphere.

If in such a search we are to be guided by terrestrial physics, we must look to those phenomena which take place in our own atmosphere at the greatest elevation above the surface of the earth.

Now the aurora will undoubtedly occur to us as an appearance which both observation and experiment induce us to associate with the extreme limits of our atmosphere.

But before discussing the question whether the auroral light presents those characteristics which observation has assigned to the red protuberances, I may remark that there is another reason, besides our unwillingness to extend the solar atmosphere, which might induce us to resort to this explanation.

The observations of General Sabine, combined with those of Dr. Schwabe, give us good grounds for supposing that the sun exercises a magnetic influence upon the earth. Now the writer has endeavoured to show that the terrestrial aurora is the induced effect in the upper regions of the atmosphere of small but rapid changes occurring in the intensity of the earth's magnetism.

These changes form what is known as magnetic disturbances, and occur simultaneously with auroras; and it has been shown by General Sabine that these disturbances have a daily period, thus discovering their dependence upon the sun, and also a ten-yearly period coincident with that of the relative frequency of sun-spots. There is therefore little doubt left in the mind that these changes in the earth's magnetism are due to similar changes in the sun's magnetic effect, and that both are connected, directly or indirectly, with those spots which appear on the surface of our luminary.

But are we to imagine that those changes in the sun's magnetic effect which, acting through the earth, produce an aurora in our atmosphere, have no similar effect upon his own?

Being convinced that we had here grounds for a hypothesis regarding the red flames, I ventured to mention this suggestion regarding their nature to General Sabine, and was much delighted to learn that the same thought had also occurred to him, which he expressed in the following words:—"May not a solar aurora call forth simultaneous corresponding auroras in all the planets, proportioned to their strength, although it may be very difficult, if not impossible, for us to determine this by observation?"

It would thus appear that, *à priori*, we have grounds for supposing that phenomena similar to terrestrial auroras may occur at the extreme limits of the solar atmosphere.

The question now arises, Can we suppose the red flames to be these auroras? have they the requisite characteristics?

There are indeed many points of likeness between the two.

In the first place, the extreme height to which the flames rise above the surface of the sun has already been adduced in favour of this hypothesis.

Secondly. The red flames have been shown by Mr. De la Rue to possess great actinic effect, a quality which Dr. Robinson and others have also found in light of the terrestrial aurora.

Thirdly. Their red or violet colour is also in favour of the hypothesis.

Fourthly. Their light is unpolarized, which is also the case with that of the aurora.

And fifthly. Some of the red flames present a curved appearance, similar perhaps to the auroral arch.

It would therefore appear that the comparison is in favour of this view, which has also the claim upon our attention that it may possibly prove of service as a working hypothesis in the science of terrestrial magnetism.

Kew Observatory,
20th Sept. 1862.

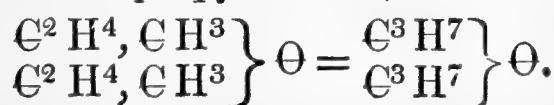
I am, Gentlemen,
Your obedient Servant,
BALFOUR STEWART.

XLI. *Chemical Notices from Foreign Journals.*

By E. ATKINSON, Ph.D., F.C.S.

[Continued from p. 192.]

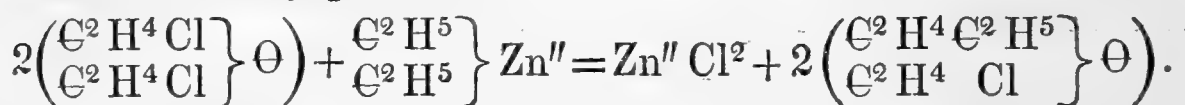
IF it were possible in methylic ether, $\left. \begin{smallmatrix} \text{C}^{\text{H}^3} \\ \text{C}^{\text{H}^3} \end{smallmatrix} \right\} \Theta$, to replace two atoms of hydrogen in the methyle by the radical methyle, a compound would be obtained having the composition of ordinary ether, $\left. \begin{smallmatrix} \text{C}^{\text{H}^2} \text{C}^{\text{H}^3} \\ \text{C}^{\text{H}^2} \text{C}^{\text{H}^3} \end{smallmatrix} \right\} \Theta = \left. \begin{smallmatrix} \text{C}^2 \text{H}^5 \\ \text{C}^2 \text{H}^5 \end{smallmatrix} \right\} \Theta$. Similarly, from ethylic ether might be obtained propylic ether,



And it would thus be possible, starting from the homologous series of ethers poor in carbon, to pass to those with a higher proportion of carbon. Lieben and Bauer* have undertaken an investigation in this direction. They caused the zinc compound of the alcohol radical to act on the chlorinated ether, and commenced their experiments with the monochlorinated ethylic ether, $\left. \begin{smallmatrix} \text{C}^2 \text{H}^4 \text{Cl} \\ \text{C}^2 \text{H}^4 \text{Cl} \end{smallmatrix} \right\} \Theta$, which is obtainable in large quantities. This body acts very energetically on zincethyle, and it is necessary that

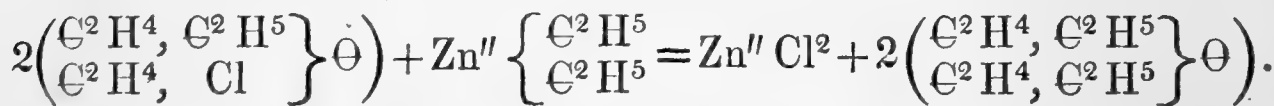
* Liebig's *Annalen*, July 1862.

the action take place at a low temperature. The product of the reaction was colourless; it was shaken with water, by which the excess of zincethyle was decomposed, and the unattacked liquid separated from the lower aqueous layer. On fractional distillation no substance of quite constant boiling-point was obtained, and the product was accordingly shaken with potash, by which some chloride of potassium was separated; and, after washing and drying, the liquid was fractionally distilled. The greater part of the product passed at 137° , and formed a limpid liquid, which, by analysis and vapour-density determination, was found to have the formula $\text{C}^6\text{H}^{13}\text{Cl}\Theta$. It has a pleasant aromatic odour, burns with green flame, mixes with alcohol and ether in all proportions, and is insoluble in water. Its rational formula, as deduced from the vapour-density, is $\left. \begin{smallmatrix} \text{C}^2\text{H}^4\text{C}^2\text{H}^5 \\ \text{C}^2\text{H}^4\text{Cl} \end{smallmatrix} \right\} \Theta$, and its formation is thus expressed:—



By the action of monochlorinated ether on zincmethyle, a body was obtained which had quite analogous properties, and whose formation took place in the same manner. It has the formula $\left. \begin{smallmatrix} \text{C}^2\text{H}^4\text{C}^2\text{H}^3 \\ \text{C}^2\text{H}^4\text{Cl} \end{smallmatrix} \right\} \Theta$, and it boils at 117° .

Lieben and Bauer then tried to remove the atom of ether which these bodies still contain, by the further action of zincethyle on the substance $\left. \begin{smallmatrix} \text{C}^2\text{H}^4\text{C}^2\text{H}^5 \\ \text{C}^2\text{H}^4\text{Cl} \end{smallmatrix} \right\} \Theta$ in sealed tubes, at first for twenty-four hours in the water-bath, and afterwards to a temperature of 120° to 140° C. in the oil-bath. The analysis of a portion of a substance obtained, which distilled over between 114° and 130° , led to the formula $\left. \begin{smallmatrix} \text{C}^2\text{H}^4, \text{C}^2\text{H}^5 \\ \text{C}^2\text{H}^4, \text{C}^2\text{H}^5 \end{smallmatrix} \right\} \Theta$. The formation of this body is thus expressed:—



With the investigation of the chemical and physical properties of this substance, which has the composition of butylic ether, $\left. \begin{smallmatrix} \text{C}^4\text{H}^9 \\ \text{C}^4\text{H}^9 \end{smallmatrix} \right\} \Theta$, the authors are still engaged.

Rieth and Beilstein* have described the following simple method of obtaining zincethyle. An alloy of zinc and sodium is prepared by melting together four parts of zinc with one of sodium. The zinc must be heated in a Hessian crucible till it

* Liebig's *Annalen*, August 1862.

begins to distil, for only at this temperature does it unite with the sodium; otherwise, on afterwards cooling, the two metals are found quite separate. The alloy may be kept in a stoppered vessel for a long time without change.

One part of this, and $1\frac{1}{2}$ parts of iodide of ethyle, are placed in a flask connected with a Liebig's condenser one end of which dips in mercury, and also with a carbonic-acid apparatus. When the apparatus is full of carbonic acid, the flask is gently warmed in the water-bath. After a few hours the reaction is terminated, and it is only necessary to distil off and rectify the zincethyle, which can be done by slightly modifying the apparatus itself. The product obtained is almost the theoretical quantity, although gases are disengaged during the operation, which bubble through the mercury. The advantages of the method consist in its certainty and entire freedom from danger.

Malic acid, fumaric acid, and maleic acid, by fermentation with cheese, pass into succinic acid: the formation of this acid from malic acid depends on a withdrawal of oxygen; the formation from fumaric and maleic on an addition of hydrogen. In most cases of fermentation, this addition of hydrogen or withdrawal of oxygen is so frequent that these actions are well called reducing. Since the reduction of malic acid to succinic by Dessaignes and by Schmidt*, and the formation of this latter acid from maleic and fumaric† by Kekulé, no doubt can exist as to the mode of formation of succinic acid in the above cases. Sugar bears the same relation to mannite that fumaric does to succinic acid.

Fumaric acid. Succinic acid.



Sugar.

Mannite.



And Linnemann‡, adding hydrogen to sugar, has changed it into mannite. For this change, cane-sugar which has been modified by treatment with dilute sulphuric acid is best fitted. A strong solution of this was made, free from acid, and small quantities of sodium-amalgam were gradually added. As soon as the liquid was alkaline, the disengagement of gas ceased; much heat was disengaged, and the vessel required to be cooled. When the action was complete, the liquid was acidulated with sulphuric acid, the excess of acid neutralized with chalk, the sulphate of lime removed by evaporation, crystallization, and the addition of alcohol to the mother-liquor. The liquid was evaporated to a syrup, from which, after long standing, the mannite separated in crystals, which both by analysis and by its general properties

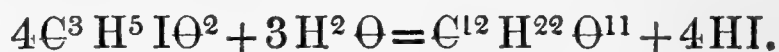
* Phil. Mag. vol. xx. p. 49.

† Ibid. vol. xxii. p. 306.

‡ Liebig's *Annalen*, July 1862.

was identified with that obtained from other sources. Linne-mann has tried the deportment of many other kinds of sugar in reference to sodium-amalgam, and in many cases observed the formation of mannite.

Beilstein, in an investigation of iodopropionic acid*, observed that, when the silver-salt of this acid was boiled, iodide of silver and a new acid, which at first was supposed to be lactic acid, were formed. He has now† more completely investigated this acid, and finds it to be totally different from lactic acid. It is best obtained by boiling iodopropionic acid with excess of oxide of silver, filtering, passing sulphuretted hydrogen through the filtrate, and evaporating to dryness. A syrup is obtained containing fine needles; and by directly neutralizing this syrup the salts may be obtained. This new acid, which Beilstein names *hydracrylic acid*, has the composition $\text{C}^{12} \text{H}^{22} \text{O}^{11}$, and in its salts is tribasic; its formation is explained as follows:—



Iodopropionic acid.

Hydracrylic acid.

Hydracrylate of lead, $\text{C}^{12} \text{H}^{19} \text{Pb}^3 \text{O}^{11}$, is a crystalline white mass which deliquesces in the air. *Hydracrylate of silver*, $\text{C}^{12} \text{H}^{19} \text{Ag}^3 \text{O}^{11}$, is very soluble in water; it is precipitated from its concentrated aqueous solution in white flocks. The characteristic property of this acid is, that, when its lead- or silver-salt is heated, it decomposes into acrylic acid and water,



Hydracrylic acid. Acrylic acid.

Beilstein subjected a quantity of the lead-salt to dry distillation; an acid liquid was obtained which had all the properties of acrylic acid, and the lead-salt of which was identical with the lead-salt of the acrylic acid prepared from acroleine.

According to Gal‡, when dry chlorine was passed for several hours into anhydrous acetic acid at a temperature of 100° , a liquid distilled over which consisted of chloride of acetylene, $\text{C}^4 \text{H}^3 \text{O}^2 \text{Cl}$, and some unchanged anhydrous acid, while monochloroacetic acid remained in the retort. Hence this decomposition is thus expressed,



Friedel has discovered§ that benzoic aldehyde, by the action of nascent hydrogen, is converted into benzoic alcohol, and that

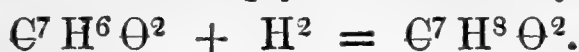
* Phil. Mag. vol. xxiii. p. 475.

† Liebig's *Annalen*, July 1862.

‡ Ibid. June 1862.

§ *Comptes Rendus*, July 1, 1862.

this is a productive method of preparing this substance. When a mixture of oil of bitter almonds and water was placed in contact with sodium-amalgam and frequently agitated, the supernatant liquor was found after some time not to combine entirely with bisulphite of soda. The oily liquid which remained after agitation with that reagent was decanted, and, on being submitted to distillation and rectification, was found to consist of benzoic alcohol, $\text{C}^7\text{H}^8\text{O}^2$. It had simply assimilated hydrogen, thus,

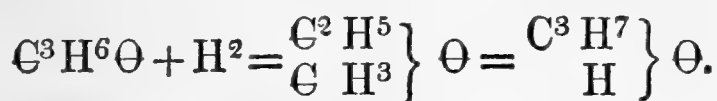


Benzoic aldehyde.

Benzoic alcohol.

In like manner Friedel found that valerianic aldehyde, $\text{C}^5\text{H}^{10}\text{O}$, is converted into valerianic alcohol, $\text{C}^5\text{H}^{12}\text{O}$.

By the action of hydrogen on acetone, there might theoretically be formed either a mixed ether or an alcohol, thus,



Acetone.

Mixed ether.

Alcohol.

Sodium-amalgam was allowed to act for some time on a mixture of acetone and water; an oily layer separated, which increased on the addition of carbonate of potash. This layer was decanted, dehydrated, and rectified. It commenced to boil at 80° , and most passed between 80° and 90° ; the temperature then rose to 175° , at which point a considerable portion of a viscous liquid was collected. The product which passed between 80° and 90° was a mixture of acetone, water, and an alcohol of the composition of propylic alcohol, $\text{C}^3\text{H}^8\text{O}$. It was further identified by conversion into the iodide, as well as by the preparation of the acetate and butyrate of propyle. Berthelot also found that the alcohol thus obtained was identical with that which he had prepared from propylene gas.

The product which in the above reaction distilled between 175° and 185° is identical with that obtained by Fittig* by the action of sodium on acetone, and which Städeler calls *pinakone*.

According to Debray†, crystallized anhydrous tungstic acid is easily obtained by heating to redness in a current of hydrochloric acid a mixture of tungstate of soda and carbonate of soda placed in a platinum boat in a porcelain tube. The tungstic acid is liberated by the hydrochloric acid, and crystallizes in the chloride of sodium simultaneously formed, in rectangular prisms. With a rapid current of hydrochloric acid, the crystals are transported to the posterior part of the tube. The properties of the crystallized acid do not perceptibly differ from those of the ordinary

* Phil. Mag. vol. xx. p. 201.

† Comptes Rendus, August 11, 1862.

acid strongly calcined. This volatilization of tungstic acid belongs to the remarkable phenomena recently discovered by M. St.-Claire Deville, concerning the action of hydrochloric acid on the amorphous oxides, and is explained in like manner*.

Tungstate of lime mixed with chalk is changed in a current of hydrochloric acid into neutral tungstate of lime, which crystallizes in the excess of chloride of calcium formed. The Scheelite thus obtained had the composition CaO WO_3 , and the crystalline form of the native mineral.

Debray also prepared tungstate of iron, or Wolfram, by heating to a high temperature a mixture in any proportions of tungstic acid and oxide of iron. All the substance was carried to the cold parts of the tube, where were deposited tungstic acid, magnetic oxide of iron, and tungstate of iron in definite lustrous crystals, identical in shape with those of native Wolfram.

Various opinions have prevailed as to the composition of the crystals formed in the sulphuric-acid chambers. Some have regarded them as a compound of sulphuric acid and binoxide of nitrogen; by others they have been considered to be a compound of sulphuric acid and nitrous acid; while Otto assumes that they contain hyponitric acid combined with sulphuric acid. In a recent research, Weltzien has adopted this view, and assigns to them the formula $6\text{SO}_2 + 2\text{NO}^4 + 4\text{HO}$.

These crystals have been examined by Dr. Weber†. The crystals were prepared by passing dry sulphurous acid into concentrated nitric acid, kept well cool until the mass had assumed a crumbly consistence; it was then allowed to drain on a dry porous tile placed under an exhausted receiver. It is important that in the preparation there shall be an excess of nitric acid, in which the crystals are not soluble.

By the action of hyponitrous acid on English sulphuric acid the same compound is obtained, though less free from adherent acid. To ascertain the composition, determinations were made of the sulphuric acid, water, nitrogen, and of the oxygen. The water was determined by placing at the end of a combustion-tube a layer of ignited magnesia. On this the weighed substance was placed, covered with magnesia, and a layer of copper 6 inches long placed in front. The apparatus being heated, a layer of perfectly dry air was passed through the tube, in front of which there was a chloride-of-calcium tube. To determine the nitrogen, the arrangement was the same as in the above case, except that, instead of air, carbonic acid was passed through the tube; and instead of the chloride-of-calcium tube, a gas-delivery

* Phil. Mag. vol. xxii. p. 515.

† *Sitzungsberichte der Berliner Akademie*, February 1862.

tube passed from the tube into a measured bell-jar over mercury. The oxygen which the substance contained, besides that which was there combined with nitrogen as binoxide of nitrogen, was estimated by its oxidizing action on protosulphate of iron.

The analytical results led to the formula $2\text{SO}^3 + \text{HO} + \text{NO}^3$, the analysis having given a slight excess of sulphuric acid and of water. The formula may be written $\text{SO}^3\text{HO} + \text{SO}^3\text{NO}^3$, which is a compound of hydrated sulphuric acid and anhydrous sulphuric and nitrous acids.

By the action of anhydrous sulphuric acid and hyponitrous acid, gas is disengaged, and a white hard crystalline compound of high specific gravity is obtained which has approximately the composition $2\text{SO}^3 + \text{NO}^3$, and is therefore the anhydrous compound. Treated with water, brown vapours are evolved, and the distillate has approximately the formula $3\text{SO}^3 + \text{NO}^3$.

XLII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 240.]

June 20, 1861.—Major-General Sabine, R.A., Treasurer and Vice-President, in the Chair.

THE following communication was read:—

“Electro-Physiological Researches.—Eleventh Series. On the Secondary Electromotor Power of Nerves, and its Application to the Explanation of certain Electro-Physiological Phenomena.” By Professor C. Matteucci.

The object of this paper is to show by experiment that when a nerve is traversed by an electric current, it acquires in all its points a secondary electromotor power, and consequently becomes capable of producing in a conducting homogeneous circuit, whose extremities touch any two points of that nerve, an electric current in an opposite direction to that of the original current. This result is independent of the vital properties of the nerves, but is affected in greater or less degree by their physical condition. A similar effect indeed is produced by the passage of an electric current in all porous substances imbibed with a conducting liquid, and the phenomenon has been studied in its generality by other physicists; but the purpose of the present paper is to determine the conditions of the secondary electromotor power of nerves, in order to make a due application of these conditions to the explanation of the phenomena exhibited by nerves on the opening of a voltaic circuit which has traversed them.

Having explained the object of his memoir, the author, before giving an account of his experiments, proceeds to describe certain improvements he has lately made in the instruments he employs for electro-physiological researches, whereby he is able more easily and effectually to avoid the risk of disturbing currents liable to be produced in the apparatus itself.

The fundamental experiment on which the main position of the

memoir rests is performed as follows :—The sciatic nerve taken from a frog, a fowl, or some other recently killed animal, is used for the purpose. The operator first assures himself that no sign of current is manifested on simultaneously touching with the galvanometer two points of the nerve equidistant from its cut extremities. The disturbing effect of the electric current naturally generated in nerves may also be eliminated by placing two nerves, or two portions of nerve, in such relative position that their natural currents shall be opposed in direction and mutually neutralize their effects on the galvanometer. To the nerve or piece of nerve thus tested are applied, at some distance from its extremities, the electrodes of a pile of eight or ten elements, and the exciting or pile-current is allowed for a short time to pass along the included part of the nerve. When the nerve is now put in communication with the galvanometer, the needle deviates, and indicates that the nerve is traversed, in the portion which had been included between the electrodes of the pile, by a current the direction of which is opposite to that of the current of the pile, and which lasts for a certain time. Signs of secondary current are also obtained by applying the galvanometer to the parts of the nerve which have not been traversed by the pile-current, that is, the end-parts between the extremities of the nerve and the points touched by the electrodes of the pile; but the secondary currents in these end-portions of the nerve are in the same direction as the pile-current, and therefore opposed to that of the secondary current developed in the part included between the electrodes and traversed by the pile-current. It is further observed, that of the two end-currents the one adjacent to the point of application of the negative electrode is stronger than the other.

It is to be noted that the secondary current endures for some time after the cessation of the exciting current; hence it is evidently not caused by induction. The author thus explains its production :—At the points of a nerve which have been acted on by the electrodes of a pile the products of electrolysation are accumulated, and thence spread through the tissue more or less, according to differences of its structure and chemical disposition; conditions, persistent for a time, are thus established for generating a current when the circuit is completed between two different points of the nerve. The same thing happens when a strip of paper or flannel, moistened with a weak saline solution, is first subjected to the current of a pile and then tested with the galvanometer, or if such a strip is so tested after having been simply wetted at one part with acid and at another with alkaline solution, to represent the effect of electrolysation by an exciting current; and in either case the direction of the secondary current in the moistened strip, both in the part included between the points of application of the electrodes and in the excluded parts at the ends, corresponds with that in the nerve.

The experiment succeeds perfectly in the entire nerve of a living animal, such as the sciatic of a rabbit or a fowl. But the result is independent of the vital condition of the nerve, for the effect is found to be equally great four days after death as at the moment an animal is killed.

The author next gives the results of experiments made to determine the influence of various physical and chemical conditions on the secondary electromotor power of nerves. The method he generally followed was by comparing the secondary currents caused by the same pile-current in a natural and in an altered nerve. He had previously ascertained that when the same pile-current is passed through two nerves at once they are equally affected, and give when placed in opposition no differential deflection of the galvanometer, although singly each might give a secondary current of 40° or 50° . Now by sending the same pile-current simultaneously through two nerves or portions of nerve, either before or after the one of them has been subjected to the particular conditions to be tried, and finally placing the two in opposition in connexion with the galvanometer, the occurrence or absence of differential deflection of the needle will show whether any or what degree of effect has been produced on the secondary electromotor power of the altered nerve. In this way the author found that both the cold of a refrigerating mixture and a heat of from 50° to 60° Cent. caused a great diminution of the secondary current in the nerves of a fowl. It is greatly weakened also by crushing the nerve, or by keeping the nerve long immersed in distilled water; immersion for a few seconds has no appreciable effect. Immersion for a few minutes in alcohol, or in a solution of potash of $\frac{1}{2000}$ strength, entirely extinguishes the secondary current. Two nerves subjected to the same exciting current, the one fifteen or twenty minutes after the other, show no difference in their secondary currents. If an exciting current is passed through a nerve, first in one direction and then for an equal time in the opposite direction, the secondary current produced is weaker than if the pile-current had been in one direction only. The secondary current increases, within certain limits, with the duration, and also with the intensity of the pile-current. Neither the size nor the number of the nerves (if united by superposition) exercises any influence. Four similar nerves were subjected to the same pile-current; three of these nerves *superposed* were then placed in opposition to one as a differential pile, but no differential current was produced. Portions of nerve of equal length from a frog, a lamb, and a fowl, were subjected to the same pile-current, and when afterwards opposed successively, the one to the other, gave no differential current; yet singly each gave a secondary current of 40° or 50° . A decided effect, however, is produced by the length of the nerve; when pieces of different lengths were compared, a strong differential current was constantly found to correspond with the longest portion of nerve; the author remarks that this result cannot be understood, unless we admit that the secondary electromotor power, which is originally greatest in contact with the electrodes, extends successively to all the parts of a nerve traversed by the pile-current.

The author then comes to the fact which he conceives to be of greatest importance, in the application of the doctrine of the secondary electromotor power of nerves to the explanation of certain electrophysiological phenomena. It is expressed in the following proposition:—

“*The secondary electromotor power of a nerve is not equal in all*
Phil. Mag. S. 4. Vol. 24. No. 161. Oct. 1862. Y

parts of the nerve, being much stronger in the portion of the nerve near the positive electrode, than in that near the negative electrode ; and this difference is greater in a nerve which has been traversed by the current in the direction contrary to that of its ramification, than in a nerve traversed by a current in the same direction as its ramification."

The unequal intensity of the secondary current and its maximum near the position of the positive electrode, is shown by cutting an electrolysed nerve into two or more equal lengths, which are then severally opposed to each other in connexion with the galvanometer, and the greatest differential current is found to be produced by the part near the situation of the positive electrode. Again, the greater effect of a pile-current passed inversely to the direction of the ramification of the nerve, is shown by opposing two nerves which have been traversed in different directions by the same current ; or by dividing a nerve and opposing its two equal halves after they have been traversed in opposite directions by the same current.

The application of these facts respecting the secondary current in nerves to the explanation of the phenomena which take place in nerves on the opening of a circuit, is treated of in the concluding part of the paper, which we give with slight abridgement in the author's own words :—

"The object of these researches," he observes, "was not to study the production of secondary electromotor power in nerves rather than in other porous and humid bodies of various structure and chemical composition. Under this point of view it is evident that the phenomenon is complex and its analysis difficult. In the present state of science, therefore, we are unable to account for the differences presented by a nerve in its different points, according to their proximity to one pole or the other, and the direction in which the nerve is traversed by the current. It is possible that similar differences will present themselves in other bodies not organized nor taken from living animals. It is sufficient for my present object to have proved that the secondary electromotor power of a nerve requires for its development the integrity of structure of the nerve itself, not, however, the excitability of the living animal ; and to have determined rigorously the differences of this power which have led me to ground the explanation of the electro-physiological phenomena which take place on the opening of the circuit, on a fundamental physical fact.

"If in a frog prepared in the usual way for electrical experiments a continuous current is passed up one hind limb and down the other, for 15, 20, or 30 seconds, according to the force of the current, it is known that the opening of the circuit is accompanied by violent contractions of the limb traversed by the inverse current. These contractions depend, as I showed many years ago*, on a particular state of the nerve ; and in fact the contractions are obtained and continue when the circuit is interrupted by cutting the nerve near the spine, but they are no longer produced if the nerve is cut near its insertion in the muscles of the leg.

"My object in this memoir has been to prove that the particular

* Phil. Trans. 1847, pp. 235, 236.

state of the nerve above described consists in secondary electromotor power, that is, in a well-known physical phenomenon. The course of the secondary current, which is downward or direct in the nerve that has been traversed by the upward or inverse pile-current, explains, according to the well-known laws of electro-physiology, the effects produced by it on that nerve on the opening of the circuit.

“The differences of electromotor power found in various points of the electrolysed nerve, the prevalence of this power in the part of the nerve near the positive electrode, very probably also the different degree of this secondary electromotor power in the various strata which compose the interior and the envelope of the nerve, are circumstances which seem to explain the secondary current which takes place in the nerve at the opening of the circuit, and which is direct and most intense in the nerve which has already been traversed by the inverse current, most intense also in the vicinity of the positive electrode.

“In order, therefore, to explain the phenomena which accompany the opening of a circuit, we must henceforth have recourse to the fact of the development of secondary electromotor power in nerves and the laws according to which it manifests itself.”

November 21, 1861.—Sir Benjamin Collins Brodie, Bart, President,
in the Chair.

The following communications were read:—

“On some Varieties of Tannin; and on Larixinic Acid, a crystallizable principle found in the Bark of the Larch Tree (*Pinus Larix*, Linn.).” By John Stenhouse, LL.D., F.R.S., F.C.S.

“On the Great Magnetic Disturbance of August 28 to September 7, 1859, as recorded by Photography at the Kew Observatory.” By Balfour Stewart, Esq., A.M. Received June 28, 1861.

During the latter part of August, and the beginning of September, 1859, auroral displays of almost unprecedented magnificence were observed very widely throughout our globe, accompanied (as is invariably the case) with excessive disturbances of the magnetic needle.

The interest attached to these appearances is, if possible, enhanced by the fact, that at the time of their occurrence a large spot might have been seen on the disc of our luminary, a celestial phenomenon which we have grounds for supposing to be intimately connected with auroral exhibitions and magnetic storms.

The auroral displays just mentioned were very attentively observed throughout Europe, America, and Australia. In many places these were of the most gorgeous character, and other places were visited by this meteor where its appearance was an event of very rare occurrence. Even from as low a latitude as Cuba we have a description of it by the Director of the Havannah Observatory, accompanied with the remark that only four previous displays had been recorded in the traditions of the island. In not a few instances telegraphic communication was interrupted owing to the current produced in the wires; and in some cases this proved so powerful that it was used instead of the ordinary current, the batteries being cut off and the wires connected with the earth. The descriptions of this meteor,

given by various observers, have been collected together by Professor Loomis, and published in a series of papers communicated by him to the *American Journal of Science and Arts*. From all these accounts there appears to have been two great displays, each commencing at nearly the same absolute time throughout the globe; the first on the evening of the 28th of August, and the second on the early morning of the 2nd of September, Greenwich time.

Magnetic disturbances of unusual violence and very wide extent were observed simultaneously with these displays. These were recorded more or less frequently at the different Observatories; but at Kew there is the advantage of a set of self-recording magnetographs, which are in constant operation, and by means of which the state of the elements of the earth's magnetic force may be known at any moment.

The author then refers to curves, which accompany his paper, and shows that the first disturbance commenced about $10\frac{1}{2}$ on the evening of August 28th, affecting all the elements simultaneously. At about $7\frac{1}{2}$ P.M., 29th August, the violence of this disturbance had somewhat abated, and things remained nearly in this state until September 2nd, 5 A.M., about which time another very abrupt disturbance simultaneously affected all the elements, and continued with great violence until about 4 P.M. of the same day, when it somewhat abated. The elements, nevertheless, remained in a state of considerable disturbance until September 5th, and only attained their normal positions on September 7th. It thus appears that we have two distinct well-marked disturbances, both commencing abruptly and ending gradually, the first of which began on the evening of August 28, and the second on the early morning of September 2.

These two great disturbances correspond, therefore, in point of time, to the two great auroral displays already alluded to.

The average effect of both of these disturbances was to increase the declination, and to diminish the horizontal and vertical components of the earth's magnetic force.

The author then alludes to a disturbance which took place about 15 minutes past 11 o'clock on the forenoon of September 1st, similar in its mode of action to the two great disturbances already mentioned, but not equalling them in extent or in duration. This disturbance affected the magnetograph simultaneously with the breaking out near a spot on the sun's disc, of a bright star, which was fortunately observed both by Mr. Carrington and Mr. Hodgson, independently.

The study of the curves furnished by the magnetograph during this great storm, in the author's opinion, seems to throw light upon the connexion which subsists between magnetic disturbances, earth-currents, and auroral displays. These curves show that at the commencement of the disturbance, the horizontal and vertical components of the earth's force remained depressed below their normal values for at least seven hours. The curves for this portion of time have also a peculiar serrated appearance, as if on the great wave of disturbance whose period was seven hours, there were superimposed smaller waves whose period might be perhaps only a few minutes, or even less.

Now Mr. C. V. Walker, in a paper communicated to the Royal Society (January 31st, 1861), shows that the earth-currents which accompany auroras are of very short period (say a few minutes), and the auroras themselves also appear to be of a very fitful nature. We cannot, therefore, well imagine how any combination of auroras and earth-currents could cause the period of seven hours, which the magnetic curves exhibit; while at the same time we cannot fail to associate the rapidly reversed earth-currents with those serrated appearances which the curves present.

The author suggests the following explanation of these phenomena. The earth itself may be likened to the soft iron core of a Ruhmkorff's machine, and the lower strata of the atmosphere to an insulating material interposed between the earth and the upper strata of the atmosphere, which, being very rare, become conducting, and form as it were the secondary coil of this arrangement.

Now suppose a primary current, probably in our luminary, to influence the earth, and suppose its general direction to remain the same for at least seven hours. This current would act on the magnetic matter of the earth in the same manner during these seven hours; and would, therefore, account for the magnetic wave of seven hours' duration.

But although this current has been supposed to remain in the same direction for seven hours, yet we may suppose that its intensity, especially if it have an atmospheric origin, is of a fluctuating character. Now any *sudden* increase or diminution in the intensity of this current, heightened by the iron core on which it acts, *i.e.* the earth, will produce secondary currents,

1st. Along the surface of the earth, which is sufficiently conducting for this purpose.

2ndly. Along the upper strata of the atmosphere, which are also sufficiently conducting.

These will be the earth-currents and auroras which, according to this hypothesis, are therefore due to the fluctuating nature of this primary current; while, on the other hand, the magnetic disturbances are due to its absolute intensity.

"On the Lead-Zinc and Bismuth-Zinc Alloys." By A. Matthiessen, F.R.S., and M. von Bose. Received August 28, 1861.

The fact that lead and zinc, and bismuth and zinc do not alloy together in every proportion is well known, but there have been, we believe, no determinations made as to the extent in which these metals alloy with each other.

The following experiments were made to ascertain quantitatively what amount of zinc will dissolve in lead and bismuth, and, on the other hand, the amount of bismuth and lead in zinc.

The metals* were fused in a Hessian crucible over a 4-Bunsen burner, stirred with a tobacco-pipe stem for a quarter of an hour, and then allowed to remain quiet for half an hour in a fused state: during the whole time a jet of gas was directed on the surface of the melted metals. They were after this cast in a porous cell, which had been previously heated to redness in a large crucible filled with sand. It

* Purified as described in the Phil. Trans. 1860, p. 177.

was generally about two hours before the metals became solid: in some cases the crucible was placed in a furnace with a low fire so as to cool much slower, but the separation of the metals did not appear to be more perfect than when cooled in the usual manner. When cold the cell was broken, and the top of the alloy separated from the lower part by a blow of the hammer.

The weight of each casting was about 300 grms., its height about 100 millims., and its diameter about 25 millims.

Lead-Zinc Alloys.

The Zinc-end.—About 12 grms. were taken from the middle of the end, avoiding the outside. The greater part of the zinc was dissolved in dilute hydrochloric acid, the residue dissolved in nitric acid and precipitated by sulphuretted hydrogen; the washed sulphide oxidized by fuming nitric acid, and the lead weighed as sulphate. Or, instead of precipitating by sulphuretted hydrogen, the mixed nitrates were precipitated by ammonia and carbonate of ammonia, and the lead weighed as oxide.

The Lead-end.—8 grms. were dissolved in nitric acid, precipitated by sulphuretted hydrogen, the filtrate evaporated almost to dryness, and again treated with sulphuretted hydrogen; the zinc then thrown down by carbonate of soda and weighed as oxide.

Analyses of the Lead-zinc Alloys.

Lead 50 per cent., and zinc 50 per cent.			
		Found.	Per-centage.
Taken lead-end... 8.000 grm.		0.162 ZnO	1.62 Zn
Taken zinc-end... 12.450 „		0.164 PbO	1.22 Pb
Lead 66.6 per cent., and zinc 33.3 per cent.			
Taken lead-end... 8.000 grms.		0.162 ZnO	1.62 Zn
Taken zinc-end... 12.55 „		0.162 PbO	1.20 Pb
Lead 4 per cent., and zinc 96 per cent.			
Top	14.170 grms.	0.247 PbO SO ³	1.20 Pb
Bottom	19.850 „	0.340 „	1.17 „
Lead 96 per cent., and zinc 4 per cent.			
Top	8.000 grms.	1.63 ZnO	1.63 Zn
Bottom	8.000 „	1.79 „	1.79 „

Bismuth-Zinc Alloys.

The Bismuth-end.—About 3 grms. were dissolved in nitric acid, evaporated with hydrochloric acid to destroy the nitric acid, precipitated by sulphuretted hydrogen, evaporated almost to dryness, and again treated with sulphuretted hydrogen; the zinc was lastly precipitated by carbonate of soda and weighed as oxide. The analyses marked (*) were made in the following manner:—The alloy was dis-

solved in nitric acid, diluted with water, and the bismuth precipitated by ammonia and carbonate of ammonia. The bismuth was filtered off and washed with a mixture of 10 parts water, 2 of ammonia, and 1 of carbonate of ammonia. The filtrate was mixed with carbonate of soda in excess, and evaporated down on a water-bath in a platinum dish; when dry, it was moistened with water and again evaporated to dryness in order to drive off the last traces of ammoniacal salts. The dry mass was then washed into a beaker, made slightly acid, boiled, and whilst boiling precipitated by carbonate of soda: the zinc was weighed as oxide.

The Zinc-end.—About 12 grms. were taken, the zinc partly dissolved out by dilute hydrochloric acid, the residue dissolved in nitric acid, and precipitated by ammonia and carbonate of ammonia: the bismuth was weighed as oxide.

Analyses of the Bismuth-zinc Alloys.

Bismuth 50 per cent., and zinc 50 per cent.			
Taken.		Found.	Per-centage.
Zinc-end	15.520 grms.	0.420 BiO ³	2.42 Bi
Zinc-end	13.990 „	0.386 „	2.48 „
Bismuth-end.....	8.000 „	1.385 „	13.85 Zn
Bismuth 50 per cent., and zinc 50 per cent.			
Zinc-end	14.2042 grms.	0.3784 BiO ³	2.39 Bi
*Bismuth-end ...	3.5060 „	0.3795 ZnO	8.65 Zn
Bismuth 80 per cent., and zinc 20 per cent.			
Top	2.990 grms.	0.525 ZnO	14.0 Zn
*Top	3.0096 „	0.5295 „	14.1 „
Bottom.....	3.694 „	0.597 „	12.93 „
*Bottom.....	2.9758 „	0.4885 „	13.1 „
Bismuth 80 per cent., and zinc 20 per cent.			
*Top	2.5542 grms.	0.5208 ZnO	16.3 Zn
*Bottom.....	2.5356 „	0.4206 „	13.3 „
Bismuth 80 per cent., and zinc 20 per cent.			
*Top	2.5778 grms.	0.4548 ZnO	14.1 Zn
*Bottom.....	2.5757 „	0.2830 „	8.8 „
Bismuth 5 per cent., and zinc 95 per cent.			
Top	13.230 grms.	0.351 BiO ³	2.38 Bi
Bottom.....	16.050 „	0.430 „	2.40 „

From the foregoing analyses, it appears that lead will, under the conditions detailed above, dissolve only 1.6 per cent. zinc, and zinc 1.2 per cent. lead; that zinc will only dissolve 2.4 per cent. bismuth, and bismuth 8.6–14.3 per cent. zinc.

If we now take equal parts of lead and zinc, fuse them together, stir them well, and cool the alloy rapidly, we may regard such an alloy as a mechanical mixture of solutions of 1·2 per cent. lead in zinc, and 1·6 per cent. zinc in lead. And the same may be said of the bismuth-zinc alloys when fused together in proportions greater than those in which these metals dissolve in one another.

“On some Gold-Tin Alloys.” By A. Matthiessen, F.R.S., and M. von Bose. Received August 28, 1861.

It was observed in a former research* that the gold-tin alloys had a great tendency to crystallize, and it was deduced from results then obtained that some of them were chemical combinations. With a view to ascertain whether these would crystallize out from the fused metals, the following experiments were undertaken.

The metals† were weighed out in the proper proportions, and fused together in a crucible over a 4-Bunsen burner, a jet of gas playing on the surface from above to prevent the oxidation of the tin. When fused, the lamp was removed and the alloy allowed to cool (the jet of gas still playing upon it) until the surface began to solidify, when the liquid alloy was poured off from the crystals. Of course the two metals were always stirred well, and cast several times before the alloy was crystallized and analysed.

When the metals were fused together in the proportion to form Au Sn_2 (62·9 per cent. Au) and Au Sn_3 (53·1 per cent. Au), no crystals could be obtained in either case. When, however, more tin was added, so as to make the alloy Au Sn_4 (45·9 per cent. Au), a separation took place into a non-crystalline mass with a glassy fracture and a very crystalline one: these may be easily separated from each other by fusion, for the former has a much higher fusing-point than the latter. The alloy containing 43·5 per cent. gold behaved in the same manner. The analyses of the different parts of these alloys are given in the following Table. The gold was determined by dissolving the alloy in nitro-hydrochloric acid, and precipitating the gold from the strong hydrochloric acid solution with sulphite of soda:—

Composition of alloy.	Analysis of non-crystalline part in per cent. of gold.			Analysis of crystals.		
	1‡	2‡	3‡	1st crop.	2nd crop.	3rd crop.
45·9 Au 54·1 Sn	47·7	48·3	49·0	43·3	43·3	42·5
43·5 Au 56·5 Sn	47·0	43·3	43·1	41·5

The weight of the alloy employed for the first experiment was about 300 grms., of which the non-crystalline part weighed about

* Phil. Trans. 1860, p. 170.

† These were purified as described in the Phil. Trans. 1860, p. 177.

‡ This was re-fused in order to remove more completely the crystallizable part of the alloy; and as soon as a part had solidified, the rest was poured off. That which first solidified was analysed.

150 grms.: in the second experiment the weight of the alloy was about 250 grms., and the non-crystalline part weighed about 50 grms.

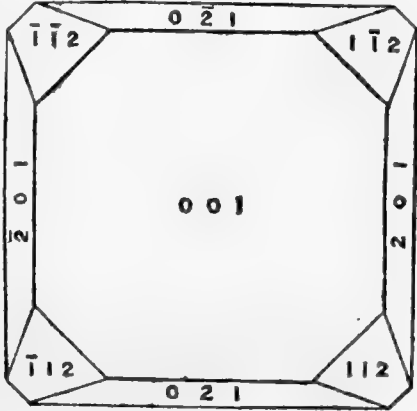
The addition of more tin did not cause any further separation into two masses. The following Table gives the results of the experiments made with the other alloys:—

Composition of alloy.	Amount of gold per cent. in first crop of crystals.	Amount of gold per cent. in	Amount of gold per cent. in mother-liquor.
41.8 Au } 58.2 Sn }	43.6	2nd crop. 43.6	40.8
40.5 Au } 59.5 Sn }	42.9	6th crop. 38.7	
37.5 Au } 62.5 Sn }	39.7	4th crop. 37.6	32.9
65.0 Au } 35.0 Sn }	37.5	4th crop. 32.6	30.6
32.5 Au } 67.5 Sn }	36.8	4th crop. 35.2	28.7
30.0 Au } 70.0 Sn }	33.8	6th crop. 31.5	25.3
25.0 Au } 75.0 Sn }	27.4	20.1

The weight of each of the alloys experimented with was generally about 260 grms., and that of each crystallization on the average 40 grms. In the last alloy, crystals fit for analysis could only be obtained in the first crystallization. The mother-liquor was, however, crystallized four times, and the last mother-liquor analysed.

We are indebted to the kindness of Professor Miller of Cambridge for the following determination of the crystalline form of the foregoing alloys:—

Description of the Crystals of Gold and Tin.



Pyramidal:

$$0\ 0\ 1, \ 1\ 0\ 1 = 50^\circ\ 2' \cdot 7.$$

Observed forms:

0 0 1, 1 0 2, 1 0 4, 3 0 4, 2 0 1, 1 1 2, 1 1 4,
with very obscure traces of the forms

$$5\ 0\ 2, \ 4\ 0\ 1, \ 2\ 0\ 3.$$

The combination most frequently observed was

$$0\ 0\ 1, \ 2\ 0\ 1, \ 1\ 1\ 2.$$

The angles between normals to the faces, computed from a mean of the best observations, are—

0 0 1,	1 0 4	=	16° 37'
0 0 1,	1 0 2	=	30 50
0 0 1,	2 0 3	=	38 31
0 0 1,	3 0 4	=	41 50
0 0 1,	2 0 1	=	67 16
0 0 1,	5 0 2	=	71 28
0 0 1,	4 0 1	=	78 10
0 0 1,	1 1 4	=	22 53
0 0 1,	1 1 2	=	40 10
2 0 1,	0 2 1	=	81 25
1 1 2,	1 1 2	=	54 16
2 0 1,	1 1 2	=	44 17

The faces of the form 0 0 1 are large and bright; those of all the other forms are extremely narrow, and are usually uneven.

Cleavage 0 0 1, very perfect.

No other forms of crystals than those just described were observed in any of the alloys experimented with.

The largest and best-defined crystals were obtained from the alloys containing about 41 per cent. of gold. The plates were sometimes, when crystallized from 300 grms., about 30 millims. long and 15 millims. wide, being the width and depth of the alloy in the crucible; they were generally of a bronze colour, proceeding from a slight oxidation of the tin: their true colour was that of tin. All the alloys emit a grating sound when cut through, as tin does, and are all exceedingly brittle.

From the above experiments it appears, first, that the well-defined crystals are not limited to one definite proportion of the constituents of the alloy, but are common to all gold-tin alloys containing from 43 to 27·4 per cent. gold; secondly, that crystals and mother-liquor are never of the same composition. These facts coincide with those found by Cooke* in his research on tin and antimony alloys, who observed that zinc and antimony are capable of uniting and producing definite crystalline forms in other proportions than those of their chemical equivalents.

“On the Sensory, Motory, and Vaso-Motory Symptoms resulting from the Refrigeration of the Ulnar Nerve.” By Augustus Waller, M.D., F.R.S.

* Silliman's American Journal, (2) vol. xx. p. 222.

GEOLOGICAL SOCIETY.

[Continued from p. 241.]

June 18, 1862.—Professor A. C. Ramsay, President, in the Chair.

The following communications were read:—

1. “On the Mode of Formation of some of the River-valleys in the South of Ireland.” By Professor J. B. Jukes, F.R.S., F.G.S., of the Geological Survey of Ireland.

Mr. Jukes’s paper contained a description of the physical structure of that part of the South of Ireland south of the limestone-plain that extends from Dublin to Galway Bay. He showed that the Rivers Shannon, Barrow, Nore, and Suir, after traversing this low ground, escaped to the sea by ravines worn through lofty hills of Old Red Sandstone and Lower Silurian rocks. He also instanced the Rivers Blackwater, Lee, and Bandon as each suddenly deserting the low longitudinal valleys through which they had run for many miles, and turning at right angles down ravines of Old Red Sandstone, notwithstanding the fact of the longitudinal valleys being continued with no apparent obstruction to the course of the rivers. He showed the connexion of these lateral ravines with the coming of strong brooks from the higher ridges on the north into the longitudinal valleys, and also that these brooks probably produced the ravines, having first begun to erode them over a surface above the present ridges, and before the formation of the longitudinal valleys.

He considered the fact proved, that the present “form of the ground” in the South of Ireland was produced by atmospheric erosion on dry land, and that the limestone ground was low because the rock had been dissolved chemically as well as eroded mechanically, and that its surface had sunk to a lower level than the other rocks, like that of a glacier melting in its bed. He proposed to extend this explanation generally to all dry land.

2. “Experimental Researches on the Granites of Ireland.—Part III. On the Granites of Donegal.” By the Rev. Professor S. Haughton, M.A., F.R.S., F.G.S.

The author described in detail the geographical position, physical structure, geological relations, and the chemical and mineralogical composition of the granite of Donegal, which consists of four minerals—quartz, orthoclase, oligoclase, and black mica, with perhaps an unknown paste besides. The oligoclase affords evidence of the probable identity of the granite of Donegal with that of Northern Scotland and of Norway. The author also alluded to his success in obtaining a formula for the determination of the proportions of *four* minerals in a compound rock, from the relative specific gravities of the mass and of its constituents.

3. “On a Stalk-eyed Crustacean from the Coal-measures.” By Professor T. H. Huxley, F.R.S., Sec.G.S.

This specimen, in an ironstone nodule, is crushed laterally, and exhibits a side view of a Crustacean, similar in all essential respects to *Pygocephalus*. The chief interest attaching to the specimen lies in the confirmation which it affords of the author’s interpretation of

the specimens on which the genus was founded. He draws the attention of collectors to the occurrence of Crustaceans of such high rank in Carboniferous rocks.

4. "On the Premolar Teeth of *Diprotodon*, and on a New Species of that Genus from Queensland in Australia." By Professor Huxley, F.R.S., Sec. G.S.

Among a collection of fossil bones from the Darling Down district, in the possession of Dr. Cotton, F.G.S., the author has observed a portion of the right ramus of the lower jaw of *Diprotodon*, and parts of the right and left upper jaws, with the anterior grinders in place, of distinct individuals. Hence he was enabled to offer some observations on the dentition of the genus, and more particularly upon the characters of the premolars. For the form which he finds distinct from *Diprotodon australis* he proposes the name of *D. minor*.

5. "On the Old Red Sandstone of Fifeshire." By James Powrie, Esq., F.G.S.

Having again examined some sections of the Old Red at White-ness, near Arbroath, and elsewhere, the author is satisfied of a local unconformity of the Upper on the Lower Old Red, but that no other locality in Forfarshire exhibits this want of conformity; and neither in Fifeshire nor Perthshire does the author find a section distinct enough to exhibit such a break in the series. Mr. Powrie alluded to the yellow sandstone of Dura Den, and observed that, though it is unconformable to the red sandstone beneath, yet he believes that it belongs to the Old Red Series. He proceeded to notice the fossil fishes of Dura Den, of which he says there are six well-marked genera (including *Glyptolepis*) with about seven or eight species.

6. "On some Upper Coal-measures, containing a bed of Limestone, at Catrine in Ayrshire." By E. W. Binney, Esq., F.R.S., F.G.S.

Some red and purple strata near Catrine, underlying the Permian breccia of Ballochmoyle, were referred to in 1856 by the author. He has since revisited the locality, and finds that these strata at Ballochmoyle Braes, Catrine, and Sorn represent a coal-field as high as any in the English series—in fact, similar to those at Ardwick near Manchester, Uffington, Leebotwood near Shrewsbury, Buxtaby near Nuneaton, and Lane End Potteries. Mr. Binney referred to the observations made by Mr. Ralph Moore, and by Geikie and Murchison, and pointed out how far he differs from them. Mr. Moore gives 565 fathoms for the whole series in Ayrshire; the author finds reason to add nearly 300 fathoms of Carboniferous strata (not productive of coal) to the above estimate.

7. "On the Geological Structure of the Southern Grampians." By Professor James Nicol, F.R.S.E., F.G.S.

The author stated that in 1844, and in subsequent years, he indicated that the Silurian strata of the South of Scotland are represented in the North by the metamorphosed or so-called primary strata; and he proceeded to point out that the object of the present communication is to examine the relation which the three great formations Clay-slate, Mica-slate, and Gneiss bear one to the other

as regular constituents of the crust of the earth, and especially in certain parts of the Scottish Highlands, as illustrated by sections observed by himself. These he correlated with what is seen in other parts of the Highlands.

Contrasting his published sections with the corresponding ones given by Sir R. I. Murchison and Mr. Geikie, he observes that, though represented as maintaining the identity of the gneiss of the west coast with certain mica- or chlorite-slates, yet he has in former papers, and in his published map, always regarded them as being identical only so far as both belong to the great series of metamorphic formations inferior to the red sandstone and quartzite, but still as distinct formations with peculiar features, and, it may be, of widely different age.

8. "On some Natural Casts of Foot-prints from the Wealden of the Isle of Wight, and of Swanage." By S. H. Beckles, Esq., F.R.S., F.G.S.

Some of these natural casts are nearly $3\frac{1}{2}$ feet long, indicating not merely the imprints of the toes, but also of the sloping metatarsals. The animal must have been of great size and weight, leaving deep imprints. Little trifold imprints of only 3 inches in length, with a stride of about 13 inches, occurred to the author also in the Isle of Wight. He has found, also, trifolds of the usual size in the Wealden of Swanage Bay. Mr. Beckles argued that other Dinosaurs besides the *Iguanodon* have left these track-marks; and he stated that from the first he has been accustomed to associate them with the various phalangeal bones so abundant in the Wealden.

9. "Geological Notes on Zanzibar." By Richard Thornton, Esq. In a letter to Sir R. I. Murchison, F.R.S., F.G.S.

From the coast to the coast-range (600 to 1300 feet high), the country consists of a series of strata with an easterly dip, namely (from above downwards) coral-limestone, sandstone, yellow shale, and sandstone with plant-remains. The mountain Kilimanjaro is formed chiefly of volcanic rocks. White and altered sandstones, with easterly dip, are met with also in the Massai Plain.

10. "On a Section at Junction-road, Leith." By W. Carruthers, Esq., F.L.S.

The author stated that in the section of clay, sand, and gravel near Leith, described by Mr. Geikie as part of a raised beach elevated since the period of the Roman occupation, not only have mediæval pottery and tobacco-pipes been found in the pottery-bearing deposit described by Mr. Geikie, but a mediæval jar has been met with in the sand beneath. The so-called "Roman" pottery was stated by the author to be of mediæval age, on the independent authority of Messrs. Birch and Franks, of the British Museum; and he believes that the beds in question are mainly of late and artificial formation; he does not, however, argue from this that there is no evidence of a late upheaval of the central part of Scotland.

11. "On the Death of Fishes in the Sea during the Monsoon." By Sir William Denison, Governor of Madras, &c. In a letter to Sir Roderick Murchison, F.R.S., F.G.S.

Steaming between Mangalore and Cananore on the west coast of India, the author found that for some time after the south-west monsoon the sea was offensive with dead fish, killed by the great mass of fresh water poured into the sea during the season of the monsoon.

XLIII. *Intelligence and Miscellaneous Articles.*

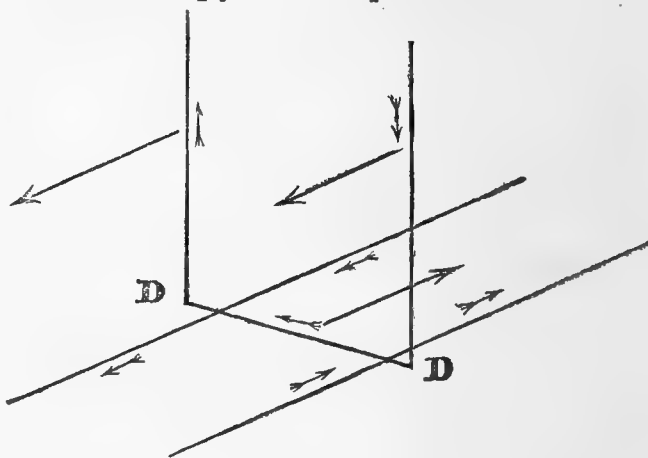
AMPÈRIAN REPULSION.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

IT would appear, from a letter of Professors van Breda and Logeman's, published in your Magazine for August last, that they are still under the impression that the apparatus described and illustrated by them in their former letter, was constructed upon sound principles, and that they attribute my objections to some misunderstanding on my part in regard to its construction.

The accompanying figure is an exact copy of the position of their conductor in relation to the currents in the mercury. If the figure is correct, certainly a mere glance will suffice to convince any one acquainted with the law of angular currents, that the horizontal part DD of the conductor is impelled in a contrary direction to the perpendicular parts, as is represented by the larger arrows, and consequently motion under these circumstances is absolutely impossible.



I am, Gentlemen,

Your most obedient Servant,

Glasgow, September 3, 1862.

JAMES CROLL.

ON THE CHANGES IN THE APPARENT SIZE OF THE MOON.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

I observe in the Philosophical Magazine for May, a letter addressed to Professor Tyndall by Mr. Richard T. Lewis, containing an explanation of the changes in the apparent size of the moon derived from a binocular examination of a lunar stereograph. If Mr. Lewis will turn to the Philosophical Magazine for January 1852*, he will find the same explanation given by Sir David Brewster in his article "On New Stereoscopes."

In fig. 4 of Plate II. in that article a particular form of a stereograph is given, which, when placed in the stereoscope, affords an

* No. 15, vol. iii. pp. 19, 20.

ocular demonstration of the cause of the changes in the apparent size of the moon.

A stereoscopic slide, copied from the figure referred to, forms No. 27 of the white-lined diagrams upon a black ground, published in Paris soon after the appearance of the article in the *Philosophical Magazine*.

In pointing out the value of the stereoscope in the investigation of difficult and important questions of a physical and metaphysical nature, Sir David Brewster has referred more fully to this experiment in his '*Treatise on the Stereoscope*,' pp. 200-204, published in 1856.

I am, Gentlemen,

Yours, &c.,

September 13, 1862.

ALPHA.

HISTORY OF THE DYNAMIC THEORY OF HEAT.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

Mr. Joule, in his "Note on the History of the Dynamical Theory of Heat," mentions Locke as the first who regarded heat as motion; but I am inclined to think that Lord Bacon has a prior claim to the theory.

In the 20th Aphorism of Book II. of the *Novum Organum* he says*, "The nature whose limit is heat appears to be motion. This is chiefly exhibited in flame which is in constant motion, and in warm and boiling liquids which are likewise in constant motion. It is also shown by the excitement or increase of heat by motion as by bellows and draughts, and by the extinction of fire and heat upon any strong pressure which restrains and puts a stop to motion." Among these far-fetched and quite inapplicable examples we find one which is used in the most recent text-books. Other species of motion produce heat, he says, and refers the reader to Aph. 13, No. 31:—"The anvil becomes so hot by the hammer, that if it were a thin plate it might probably grow red, like ignited iron, by repeated strokes."

Bacon defines heat as follows, "heat is not a uniform expansive motion of the whole, but of the small particles of the body; and this motion being at the time restrained, repulsed, and reflected, becomes alternating, perpetually hurrying, striving, struggling, and irritated by the repercussion, which is the source of the violence of flame and heat."

Bacon's inquiry concerning the nature of heat has been condemned. It is true that his reasoning is in many points at fault, that his examples are badly chosen, and often do not apply: but when we consider the amount of physical knowledge in the world in Bacon's time, and that this was the first attempt at arranging together all the properties of a force and inferring the nature of that force from its properties, and, lastly, that Bacon came to the conclusion that heat is a kind of motion, a theory now generally accepted,—when we

* According to J. Devey's translation.

remember this, we must allow that, however erroneous some of the conclusions arrived at may be, still Bacon's inquiry was a valuable "suggestion for the interpretation of Nature," and a guiding-post placed in the gloomiest and most obscure part of the road of science.

I am, &c.,

GEORGE F. RODWELL.

REDUCTION OF CHROMIUM FROM SOLUTION OF THE CHLORIDE.

BY CHARLES W. VINCENT.

In the course of some experiments with amalgam of sodium, the idea occurred to me that it might be employed with advantage as a ready means of reducing some of those metals which are not readily obtained by ordinary metallurgical processes. By adding to a solution of the chloride of chromium an amalgam of sodium, I have found that, although there is a considerable waste of sodium, nevertheless an amalgam remains of chromium, which, on distillation in a tube-retort filled with naphtha vapour, yields this metal in a finely-divided state.

The accompanying Note is an account of the reaction which takes place with manganese. The subject is still under investigation.

2 Greyhound Court, Milford Lane, W.C.

REDUCTION OF MANGANESE FROM THE PROTOCHLORIDE BY
SODIUM-AMALGAM. BY W. B. GILES.

When an amalgam of sodium is placed in a saturated solution of pure protochloride of manganese, a rapid action takes place, hydrogen is evolved, and finally an amalgam of manganese remains. The amalgam appears to decompose slowly in presence of water; and if exposed to the air for a short time, becomes covered with a brown film. The amalgam is then taken out of the solution of chloride, strongly pressed to get rid of the fluid mercury, and then placed in a hard glass tube closed at one end. The tube is then heated till all the mercury is driven off. There remains in the tube a brownish-black powder, which appears to be manganese. If poured from the tube while still rather hot, the manganese takes fire, glowing like tinder till it is all converted into oxide. It also exhibits a splendid combustion, resembling that of iron filings when sprinkled into the flame of a spirit-lamp or Bunsen's burner. A portion was poured from the tube while still hot, and allowed to burn; when cold it was dissolved in hydrochloric acid, giving a red solution of sesquichloride of manganese, which by boiling was converted into protochloride.

The clear solution was divided into two portions: to one, sulphide of ammonium was added; the flesh-coloured sulphide of manganese was obtained; to the other, ammonia; the white hydrate of protoxide of manganese fell, turning brown in a short time. A little dry oxide was fused with carbonate of potassa before the blowpipe; green manganate of potassa was immediately obtained. From these experiments I infer that the powder is metallic manganese.

The same results appear to take place with cobalt.

St. Paul's Terrace, Ball's Pond,

THE
LONDON, EDINBURGH, AND DUBLIN
PHILOSOPHICAL MAGAZINE
AND
JOURNAL OF SCIENCE.

[FOURTH SERIES.]

NOVEMBER 1862.

XLIV. *On the Thermal Phenomena which accompany the changes in volume of Solid Bodies, and on the corresponding Mechanical Work.* By M. EDLUND*.

IT follows directly from the first principles of the mechanical theory of heat, that the absorption or disengagement of heat which accompanies any modification of a body, does not depend solely on this modification itself, but on the external mechanical work which may be created or destroyed at the same time as this modification takes place. The experimental verifications of this general theorem are, with reason, classed amongst the most convincing proofs which can be given of the great principle of the equivalence of mechanical work and heat. Thus, in late years, physicists have laid great weight on Hirn's experiments on the heat disengaged by the condensation of the steam of a machine in motion, and on the experiments of M. Favre on the total heat disengaged by the solution of an equivalent of zinc in a voltaic circuit comprising an electro-magnetic motor. M. Edlund has proposed to himself the task of investigating, from the same point of view, a thermal phenomenon as yet little studied—the disengagement or absorption of heat which accompanies the contraction or elongation of a metal wire.

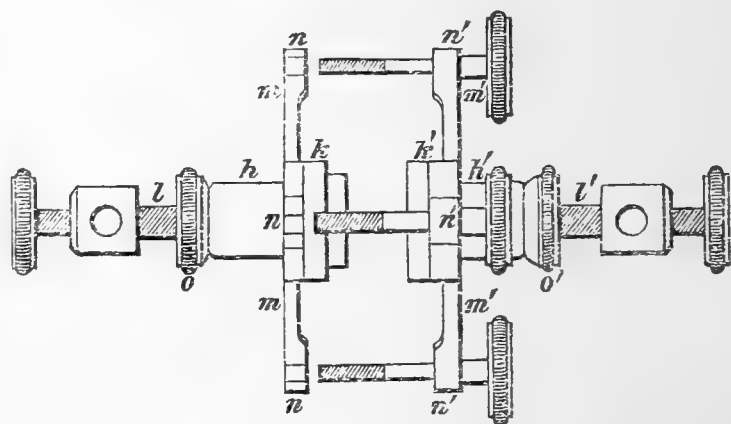
The principle of the experiments consisted in measuring successively, 1st, the quantity of heat absorbed by a wire which is elongated during the time in which a weight suspended to its extremity descends by a given quantity; 2nd, the quantity of heat disengaged by the same wire when it became shorter by the

* Translated from an abstract, published by M. Verdet in the *Annales de Chimie et de Physique*, February 1862, of the original paper, which appeared in Poggendorff's *Annalen*, vol. cxiv. p. 1.

same quantity as in the preceding case, in raising the same weight; 3rd, the quantity of heat disengaged when the wire became shortened without raising any weight. From the mechanical theory of heat, the two first quantities ought to be equal, but less than the third.

To estimate these quantities of heat, M. Edlund used a thermo-electric couple of a peculiar construction, which exactly measured the diminution or elevation of temperature of the metallic wire to which it was applied. It consisted of a crystal of antimony and a crystal of bismuth (not represented in the figure) fixed in two pieces of ivory, h and h' (fig. 1), which were themselves fixed in two large brass springs, $m m$, $m' m'$.

Fig. 1.



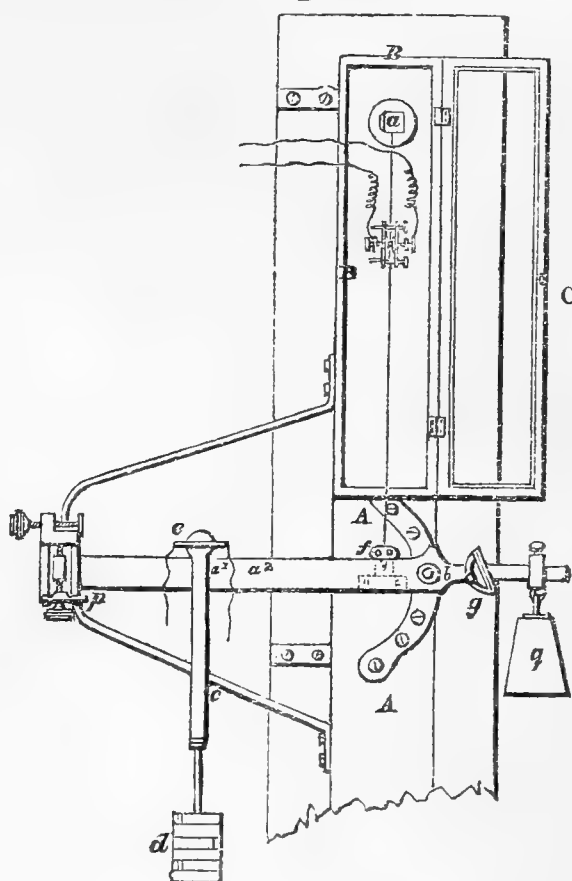
The crystals communicated on the inside with the metal screws l and l' , and by means of these screws with the galvanometer. The three screws n, n' served to bring together the two springs $m m$ and

$m' m'$, and thus to press the metal wire between the two crystals without its being otherwise in contact with any part of the apparatus. M. Edlund considers the substitution of defined crystals for the melted and confusedly crystalline bars commonly used, as an essential improvement. As the electromotive force of a bismuth-antimony couple may vary greatly with the nature of the crystalline faces in contact, if in two successive experiments the two crystals are not in contact with the metal wire in exactly the same points, the results could not be compared. A Weber's reflecting galvanometer was used with an astatic needle.

The metal wire used was fixed at the top to the end of a short bar of iron, a (fig. 2), which was itself supported by a strong vertical beam A, fixed in a recess. At the bottom there was a steel pincer f , perforated by a horizontal hole, which fitted in the two arms of a metal fork supported by a wooden lever $a^1 a^2$, moveable about a horizontal axis. When a steel rod was passed through two holes in the prongs of the fork, and through the hole of the pincer f , the lower end of the wire was virtually part of the lever. A weight q was suspended to the lever on the other side of the axis b , and was placed in such a position that the preceding operation could be effected without deranging the lever, and consequently without elongating the wire at all. A piece of brass, c , which supported a weight d , produced elongations by the intervention of the lever

$a^1 a^2$. For this purpose it was at first placed above the axis b , and then made to slide towards the end p . This motion was easily effected by the aid of a wire, as the piece c rested on the lever by a roller e , and the upper surface of the lever was carefully polished. The angular displacement of the lever could be estimated by a mirror g , in which the image of a divided vertical scale was observed by reflexion. The scale was divided into millimetres; each division corresponded to an angular displacement of the lever of $47''\cdot6$, and an elongation of the wire of 0.0114 millim. As the total length of the wire was about 0.590 metre, the accuracy of the experiments was sufficiently great.

Fig. 2.



Lastly, to exclude as much as possible accidental changes of temperature, and to retain its temperature uniform throughout its entire length, M. Edlund arranged a glass case, BC (represented open in the figure), lined inside with tinfoil.

In making an experiment, the weight d was rapidly passed from the axis b to the end of the lever; the angular displacement was noted, as well as the first range of the galvanometer needle. M. Edlund took this range as a measure of the diminution of the temperature of the wire, and established this proportionality by the following reasoning.

Let x be that division of the scale which at the moment t is brought, by reflexion on the mirror of the galvanometer, to coincide with the vertical wire of the telescope; x_0 the value of x corresponding to the time $t=0$ at which no current passes in the wire of the galvanometer; m the quotient of the magnetic moment of the needle by its moment of inertia; $2n$ a constant which depends on the induced currents developed by the motion of the needle in the conducting parts of the apparatus, and which is inversely as the moment of inertia of the needle; k another constant directly proportional to the electromotive force of the couple employed, and inversely proportional to the resistance of the circuit, as well as to the moment of inertia of the needle;

v the excess of temperature at the moment t . The differential equation

$$\frac{d^2x}{dt^2} + m(x - x_0) + 2n \frac{dx}{dt} - kv = 0$$

is easily obtained.

As, further, the excess v varies with the time independently of x , in consequence of the cooling of the wire, in conformity with Newton's law,

$$dv + avdt = 0,$$

whence

$$v = v_0 e^{-at},$$

and the differential equation becomes

$$\frac{d^2x}{dt^2} + m(x - x_0) + 2n \frac{dx}{dt} - kv_0 e^{-at} = 0,$$

the general integral of which is

$$x - x_0 = \frac{kv_0 e^{-at}}{a^2 - 2an + m} + e^{-nt} (C \cos t \sqrt{m - n^2} + C_1 \sin t \sqrt{m - n^2}).$$

As, at the time $t=0$, $x - x_0$ and $\frac{dx}{dt}$ are simultaneously zero, the two constants C and C_1 are easy to determine, and we get

$$x - x_0 = \frac{kv_0}{a^2 - 2an + m} \left[e^{-at} + e^{-nt} \left(\frac{a - n}{\sqrt{m - n^2}} \sin t \sqrt{m - n^2} - \cos t \sqrt{m - n^2} \right) \right]$$

If, in this expression, the smallest of the values which reduce the velocity $\frac{dx}{dt}$ to zero be placed for t , the corresponding value of $x - x_0$ will be the amplitude of the first range of the needle; and it will be evidently proportional to v_0 , that is, to the initial variation of the temperature of the wire.

The reasoning which leads to this conclusion supposes that the calorific phenomenon, of which the wire is the seat, is of very short duration, and that it is ended before the displacement of the needle has become sensible. But experiment shows that it is not necessary that this condition be rigorously satisfied, but that the duration of the fall of the weight may be prolonged to six seconds without the original range of the needle being sensibly diminished.

Experiment shows that, for the same wire, the amplitude of the first range is really proportional to the quantity of heat absorbed or disengaged in the wire. If, in fact, these be determined by means of four series of successive experiments—(1) the amplitude X , which corresponds to the total fall of the weight d from the axis b to the end of the lever, (2) the amplitude X_1 ,

which corresponds to a partial fall from b to a^1 , (3) the amplitude X_2 , which corresponds to a partial fall from a^1 to a^2 , (4) the amplitude X_3 , corresponding to a partial fall from a^2 to the end of the lever—we have

$$X = X_1 + X_2 + X_3 :$$

and this relation is the sure test of the proportionality of effects to causes, that is, of the proportionality of the amplitudes to the quantities of heat.

When the return of the needle to its original position indicated the return of the wire to the surrounding temperature, the weight d was caused to rise to b , and in like manner the elevation of temperature due to the shortening of the wire was noted. A second elongation, equal to the first, was obtained by a second fall of the weight d ; but when the equilibrium of temperature was re-established, instead of again making the weight ascend towards the axis, the little steel rod which made the pincer f part of the lever $a^1 a^2$ was rapidly withdrawn, the end p of the lever fell upon a support, as seen in the figure, and, the wire returning to its original length, the corresponding elevation of temperature was noted. Thus were observed the two variations of temperature successively produced by an elongation and a contraction, accompanied by external work (positive in the case of the elongation, negative in that of a contraction) equal to the product of the weight into the elongation or contraction itself—and of a third variation produced by a contraction equal to the preceding, but which had been accompanied by no external sensible mechanical work; for the work of the gravity corresponding to the very small displacement of the system formed by the wire and the pincer could be neglected as compared with the work of the two first experiments. For the same wire, and in the same series of experiments, these variations of temperature are evidently proportional to the quantities of heat disengaged or absorbed; but if it were desired to determine these quantities of heat themselves, or merely compare the quantities disengaged or absorbed by different wires, or by the same wire in different experiments, it would be necessary to take into account a certain number of influences which it is almost impossible to estimate. Thus it is clear that the manner in which heat is communicated from the wire to the pincer depends on the nature of the wire, and the pressure exerted by the pincer on the wire. This pressure even exercises another influence; it counteracts the changes in length of that part of the wire held between the two branches of the pincers, and consequently diminishes the variations of temperature which take place in this part, and which are precisely those which experiment shows.

It might be feared that the friction of the wire against the pincer would exercise a considerable disturbing influence on the results, in consequence of the heat which it develops. But the experiments show clearly that there is nothing of the kind; for they prove that the calorific effects observed when the wire lengthens or shortens in consuming or in developing the same external work are exactly equal, and of contrary sign, which could not have taken place if the heat disengaged by friction in one experiment diminished the disengagement of heat due to elongation, and in the following experiment increased the elevation of temperature due to contraction.

The first series of experiments was made with a steel wire (a piano string) of 1.14 millim. in diameter. The results are contained in the following Table, in which u , u' , u'' designate the three ranges of the galvanometer needle corresponding to the three periods of the experiment above described:—

Charge of the lever . . . 11.848*			
	u .	u' .	u'' .
	48.0	46.0	97.0
	48.0	43.0	99.0
	45.0	...	91.0
	45.0	49.0	99.0
Mean . .	46.5	46.0	96.5
Charge of the lever. . . 6.665			
	u .	u' .	u'' .
	29.0	25.0	43.5
	26.5	25.0	41.0
	...	30.0	40.0
	31.0	33.6	42.0
	34.0	26.0	
	26.0	23.0	
Mean . .	29.3	27.1	41.6
Charge of the lever . . . 8.393			
	u .	u' .	u'' .
	34.0	28.0	57.0
	35.0	28.0	53.5
	36.5	38.0	53.0
	33.5	30.0	50.0
	34.5	35.0	57.0
	30.0	40.0	56.5
Mean . .	33.9	33.2	54.5

* M. Edlund's unit of weight is the Swedish pound.

Charge of the lever . . . 10·242

<i>u.</i>	<i>u'.</i>	<i>u''.</i>
37·0	44·0	74·0
47·5	40·0	74·0
40·5	45·0	72·5
47·0	36·0	75·0
40·0	44·5	74·5
41·0	43·5	
Mean . .	42·2	74·0

Charge of the lever . . . 12·758

<i>u.</i>	<i>u'.</i>	<i>u''.</i>
58·0	56·0	122·0
60·0	50·0	112·0
56·0	59·0	113·0
57·0	55·0	117·0
49·0	54·5	115·5
...	53·5	117·0
Mean . .	54·7	116·0

The equality of the mean values of *u* and *u'* in each series of experiments is the first fact noticed in inspecting this Table. Thus the heat disengaged by contraction, and the heat absorbed by elongation, are equal *when the contraction and elongation are accompanied by external mechanical work which is equal, but of contrary sign.* The proportionality of heat disengaged to the charge of the lever is no less easy to confirm in the numbers which precede. Finally, the proportionality of the elongations to the charges is proved by another series of numbers which are not given here.

All these conclusions contain nothing very new; but it is otherwise with that which may be drawn from a comparison of the values of *u''* with the values of *u'* and *u*. The first are always higher than the second; in other terms, when the wire contracts without *effecting an external mechanical work*, more heat is disengaged than when it contracts to the same extent *in raising a weight*. It is easy to see that the expression $u'' - \frac{u + u'}{2}$ may be taken as a measure of the excess of heat disengaged in the first case over the heat disengaged in the second; and the numbers obtained by experiment show that this excess is proportional to the square of the charge on the arm of the lever, or (what is the same thing, since the elongation is proportional to the charge) proportional to the product of the charge by the elongation, that is, to the mechanical work effected in the experiment in which the

wire contracted while raising a weight. This latter result is quite in accordance with theory.

M. Edlund experimented on a second steel wire quite like the preceding, on a silver wire 1.14 millim. in diameter, on a German silver wire 1.32 millim. in diameter, on a brass wire 0.87 millim. in diameter, on a platinum wire 1.92 millim. in diameter, and on a wire of aluminium-bronze*. All these experiments have given results analogous to the foregoing, and which it appears superfluous to repeat in detail. M. Edlund sums up his conclusions in the following general laws:—

I. *If a metal is expanded without the limit of elasticity being exceeded, it undergoes a cooling proportional to the mechanical force by which the dilatation is produced.*

II. *If the metal then reverts to its original volume, and effects an external mechanical work equal to that which has been expended in dilating it, it experiences a heating equal to the preceding cooling, and therefore proportional to the mechanical force by which the metal was maintained in its state of expansion before contracting.*

III. *If, on the contrary, the metal returns to its original volume without producing any external mechanical work, it becomes more heated than in the preceding case, and the difference of the two heatings is proportional to the external mechanical work which in the one case the metal effects at the same time as it contracts.*

IV. *It follows from these three principles, that if, without exceeding the limits of elasticity, a metal passes from the volume V_0 to the volume V_1 , the accompanying calorific effect does not depend merely on the original and on the final volume, but also on the manner in which the total change has been effected.*

M. Edlund remarks that when a metal contracts without effecting an external mechanical work equal to that which has been necessary to expand it, its molecules return to their original positions of equilibrium *without velocity*, because the tension of the wire decreases in the same ratio as the elongation. On the contrary, if contraction takes place without any external mechanical work, the motion of the molecules is incessantly accelerated, and they arrive at their positions of equilibrium endowed with velocities which may be regarded as identical with the excess of heat disengaged. It might be said that the external mechanical work *hinders* the production of a certain quantity of heat which would naturally be developed.

Analogous considerations may be applied to the older experiments of Joule on the thermal effects of the expansion of gases.

* Containing 2.5 per cent. of aluminium, according to an analysis of M. Ullgren.

XLV. *On the Absorption and Radiation of Heat by Gaseous Matter.*—Second Memoir. By JOHN TYNDALL, F.R.S., Professor of Natural Philosophy in the Royal Institution.

[Continued from p. 287.]

§ 6. **I** HAVE now to refer to a class of facts which surprised and perplexed me when I first observed them. As an illustration, I will first take the case of alcohol vapour. A quantity of this substance, sufficient to depress the mercury gauge 0·5 of an inch, produced an absorption which caused a deflection of 72° of the galvanometer needle.

While the needle pointed to this high figure, and previously to pumping out the vapour, I allowed dry air to stream into the tube, and happened while it entered to observe the effect upon the galvanometer. The needle, to my astonishment, sank speedily to zero, and went to 25° at the opposite side. The entry of the almost neutral air here not only abolished the absorption previously observed, but left a considerable balance in favour of the face of the pile turned towards the source. A repetition of the experiment brought the needle down to zero, and sent it to 38° on the opposite side. In like manner a very small quantity of the vapour of sulphuric ether produced a deflection of 30° ; on allowing dry air to fill the tube the needle descended speedily to zero, and swung to 60° at the opposite side.

These results both perplexed and distressed me; for I imagined, on first observing them, that I had been throughout dealing with an effect totally different from absorption. I thought, at first, that my vapours had deposited themselves in opaque films on my plates of rock-salt, and that the dry air on entering had cleared these films away, and allowed the heat from the source free transmission.

But a moment's reflection dissipated this supposition. The clearing away of such a film could at best but restore the state of things existing prior to its formation. It might be conceived of as bringing the needle again to 0° ; but it could not possibly produce the negative deflection, which, in the case of ether vapour, amounted to the vast amplitude of 60° . Nevertheless I dismantled the tube, and subjected the plates of salt to a searching examination. I satisfied myself thus that no such deposition as that above surmised took place. The salt remained perfectly transparent while in contact with the vapour.

Some of the experiments recorded in the Bakerian Lecture for this year (1860) had taught me that the dynamic heating of the air when it entered the exhausted tube was sufficient to produce a very sensible radiation on the part of any powerful vapour contained within the tube, but I was slow to believe that the enormous effect above described could be thus accounted for. My first care was to determine the difference of temperature between

a thermometer placed within the tube at the end furthest from the source, and one placed without it. I then examined, by an extremely sensitive thermometer, the increase of temperature produced by the admission of dry air into the tube, and the decrease consequent on pumping out, and found the former to be a considerable fraction of the total heat transmitted from the source. Could it be that the heat thus imparted to the alcohol and ether vapours, and radiated by them against the adjacent face of the pile, was more than sufficient to make good the loss by absorption? The *experimentum crucis* at once suggested itself here. If the effects observed be due to the dynamic heating of the air, we ought to obtain them even when the sources of heat made use of in the experiments are entirely abolished; and we should thus arrive at the solution of the novel and at first sight utterly paradoxical problem, *To determine the radiation and absorption of gases and vapours without any source of heat external to the gaseous body itself.*

For the sake of brevity, I will call the heating of gas by its admission into a vacuum, the *dynamic heating* of the gas; and the chilling accompanying its pumping out, *dynamic chilling*. It would also contribute to brevity if I were allowed to call the radiation and absorption of the gaseous body, consequent on such heating and chilling, *dynamic radiation* and *dynamic absorption*, though I fear the terms are not unobjectionable.

§ 7. *On Dynamic Radiation and Absorption.*—Both the source of heat and the compensating cube were dispensed with, and the thermo-electric pile was presented to the end of the cold experimental tube. By a little management, the slight inequality of radiation against both faces of the pile, arising from differences in the various parts of the laboratory, was obliterated, and the needle of the galvanometer thus brought to 0°.

The vapours were admitted in the manner already described, until a tension of 0·5 of an inch was obtained. The air was then allowed to enter through a drying-apparatus by an orifice of a constant magnitude. Two stopcocks, in fact, were introduced between the drying-tube and the experimental tube; one of these was kept partially turned on, and formed the gauge for the admission of the air. When the tube was to be exhausted, the second stopcock was turned quite off. When the tube was to be filled, this stopcock was turned full on; but the *gauge-cock* was never touched during the entire series of experiments.

Before, however, the mode of experiment was thus strictly arranged, a few preliminary trials gave me the following results:—

Nitrous oxide on entering caused the needle to swing in a direction which indicated the heating of the gas; the limit of its excursion was 28°, after which it slowly sunk to 0°.

The pump was now worked; the propulsion of the first por-

tions of the gas from the tube was so much work done by the residue. That residue became consequently chilled; into it the face of the pile adjacent poured its heat, and a swing of the needle on the negative side of 0° was the consequence. The limit of the excursion was 20° .

Olefiant gas, operated on in the same manner, produced on entering the tube a swing of 67° , showing radiation; and on pumping out, a swing of 41° , showing absorption. After the pumping out of the gas, and without introducing a fresh quantity, *dry air* was again admitted; the swing produced by the dynamic radiation of the residue of the gas ($0\cdot2$ of an inch in tension) was 59° . On pumping out *very quickly*, the dynamic absorption produced a deflection of nearly 40° .

A little of the vapour of sulphuric ether was admitted into the tube; on the admission of dry air afterwards, the needle swung from 0° to 61° ; on pumping out, the needle ran up to 40° on the opposite side.

These and other experiments, which I confess gratified me exceedingly, showed that, without resorting to any source of heat external to the gaseous body itself, its radiation and absorption might be determined with extreme accuracy, and the reciprocity of both phenomena rendered strikingly clear. In fact, at this very time I had been devising an elaborate apparatus for the purpose of examining the radiation of gases and vapours, with a view to comparing it with their absorption; but no such apparatus would have given me results equal in accuracy to those placed within reach by the discovery of dynamic radiation and absorption.

The following Table is the record of a series of experiments in connexion with this subject. The vapour in each case was admitted till the mercury column fell half an inch, and dry air was admitted afterwards.

TABLE IV.—Dynamic Radiation and Absorption of Vapours.

	Deflections.	
	Radiation.	Absorption.
Bisulphide of carbon	14	6
Iodide of methyle	19·5	8
Benzole	30	14
Iodide of ethyle	34	15·5
Methylic alcohol	36	
Chloride of amyle	41	23
Amylene	48	
Alcohol	50	27·5
Sulphuric ether	64	34
Formic ether	68·5	38
Acetic ether	70	43

The paradox already referred to is here solved, and the explanation given of the extraordinary effect observed in the case of the alcohol and ether vapours when dry air entered the experimental tube. Dynamic radiation, moreover, and dynamic absorption go hand in hand; and if we compare both with Table III., we shall find the order of the substances precisely the same, although one set of results are obtained with a source of heat external to the gaseous body, and the other with a source of heat and cold within the body itself. Had I sufficient time at my disposal, I could develop this subject with advantage. The results just recorded constitute my first regular series of experiments; and, no doubt, augmented experience will enable me to attain more perfect results.

I could not well obtain half an inch of my most energetically acting vapour, namely, boracic ether; but one-tenth of an inch admitted into the tube and dynamically heated and chilled, gave—

Radiation.	Absorption.
56°	28°

Seeing the astonishing energy with which some of these vapours absorb and radiate heat, it may be asked how far the quantity of vapour may be reduced before its action becomes insensible. At present I will not venture to answer this question fully; certainly we should be dealing at least with millionths of our smallest weights. But I will here lay before the Society an account of one experiment, the result of which can hardly fail to excite astonishment. The experimental tube being exhausted, one-tenth of an inch of boracic-ether vapour was admitted into it: the barometer stood at 30 inches at the time; hence the tension of the vapour within the tube was $\frac{1}{300}$ th of an atmosphere.

Dynamically heated by dry air, the radiation of this vapour produced a deflection of 56°.

The tube was then exhausted to 0·2 of an inch, and the quantity of vapour reduced thereby to $\frac{1}{150}$ th part of its first amount; the needle was allowed to come to zero, and the residue of vapour was dynamically heated as before: its radiation produced a deflection of 42°.

The pump was again worked till a vacuum of 0·2 of an inch was obtained, this residue containing of course $\frac{1}{150}$ th of the quantity of ether present in the last. On dynamically heating this residue, its radiation produced a deflection of 20°*.

Two additional exhaustions, succeeded by dynamic heating, gave the deflections 14° and 10° respectively.

* This is less than the truth—my assistant having executed three or four strokes of the pump inadvertently while the dry air was not shut off, removing thereby a considerable proportion of the vapour which ought to be present at this stage of the experiment.

Tabulating the results so as to place each deflection beside the vapour-tension which produces it, we have the following view of the experiment :—

TABLE V.—Dynamic Radiation of Boracic Ether.

	Tension in parts of an atmosphere.	Deflection.
	$\frac{1}{300}$ th	56°
	$\frac{1}{150} \times \frac{1}{300} = \frac{1}{45000}$ th	42
	$\frac{1}{150} \times \frac{1}{150} \times \frac{1}{300} = \frac{1}{6750000}$ th	20
	$\frac{1}{150} \times \frac{1}{150} \times \frac{1}{150} \times \frac{1}{300} = \frac{1}{1012500000}$ th	14

The air itself, slightly warming the apparatus near the pile, produces a feeble radiation, amounting to 6° or 7°. I have purposely excluded the deflection 10°, in order to show that the effect was still diminishing when the experiment ended, the constant effect due to the air itself being not yet attained. I thus exclude two 0s from the denominator of my fraction which might fairly have appeared in it. The above result is, however, sufficiently extraordinary, showing as it does that the radiation of an amount of vapour possessing in our tube a tension of less than the thousand millionth of an atmosphere is perfectly measurable. It will also be borne in mind that the temperature imparted to this infinitesimal quantity of matter did not exceed 0·75 of a Centigrade degree.

These experiments, which I intend to develop on a future occasion, seem to give us new ideas as to the nature and capabilities of matter. A platinum wire raised to whiteness in a vacuum by an electric current, becomes comparatively cold in a second after the current has been interrupted; yet that wire, while ignited, was the repository of an immense amount of mechanical force. What has become of this? It has been conveyed away by a substance so attenuated that its very existence must for ever remain a hypothesis. But here is matter that we can weigh, measure, taste, and smell—that we can reduce to a tenuity which, though expressible by numbers, defeats the imagination to conceive of it. Still we see it competent to arrest and originate quantities of force which in comparison with its own mass are almost infinite, a small fraction of this force causing the double needle of the galvanometer to swing through considerable arcs. When we find common ponderable matter producing these effects, we have less difficulty in investing the luminiferous ether with those mechanical properties which have long excited the interest and wonder of all who have reflected upon the circumstances involved in the undulatory theory of light.

In the foregoing experiments dry air was used to warm the vapours, but similar differences ought to be exhibited by gases when heated by their own dynamic action. This is the case, as the following experiments show :—

TABLE VI.—Dynamic Radiation of Gases.

Name.	Radiation.
Air	7 ⁰
Oxygen	7
Hydrogen	7
Carbonic oxide	19
Carbonic acid	21
Nitrous oxide	31
Olefiant gas	63

I also satisfied myself of the energetic radiation of the two following gases, which, however, were used in irregular quantities. They were admitted into the tube from a large bolthead, until a common tension was established between the gas in the tube and the gas in the bolthead.

	Radiation.	Absorption.
Ammonia 15 in. tension . . .	56 ⁰ ·5	33 ⁰ ·5
Sulphurous acid 16 in. tension .	45	24

Let us reflect for an instant on the condition of our tube with its $\frac{1}{2}$ inch of vapour at the moment when the latter has been heated by the entrance of the air. The gaseous column is heated throughout to the same temperature; the elastic condition of the luminiferous ether is the same for all the particles, and consequently their periods of vibration are all the same. Hence each molecule is in that precise condition which enables it to absorb most effectually the undulations emanating from its neighbours. The rays from the particles at the end of the tube most distant from the pile have to cross a space of nearly 3 feet before they reach the latter, this space being partially filled with molecules circumstanced as just described. Hence absorption to a comparatively greater extent must occur; and indeed we can imagine the tube so long that its frontal portion should furnish a vapour screen absolutely opaque to the radiation of its hinder portion. Comparing ether vapour with olefiant gas, it is, I think, evident that the radiant points of the attenuated vapour which depresses the mercury column only 0·5 of an inch are further apart than those of the gas which depresses the column 30 inches. Consequently there is a wider door open for the radiation of the distant ether particles towards the pile than

for the distant particles of olefiant gas. The length of the whole column, in fact, might be available for the radiation of the vapour, and a part of it only available for the gas. Cut off this unavailable portion from the gas column, and we do not injure its efficacy; but cut off a similar length from the vapour column, and we may materially diminish its effect. Speaking generally, by reducing the column of ether and that of gas by the same amount, the diminution of radiation will be most sensibly felt where the radiant points are furthest asunder. Reasoning thus, it becomes evident that in a long tube the vapour may excel the gas in its amount of radiation, while with a short tube the gas may excel the vapour. Let us now test this reasoning by experiment.

The dynamic radiation of the following four substances has been tabulated thus:—

Sulphuric ether	64 ⁰
Formic ether	68·5
Acetic ether	70
Olefiant gas	63

The action of olefiant gas is therefore smallest when the length of the radiating column is 2 feet 9 inches.

Experiments of the same character were made with a tube 3 inches long, or of the former length, and the following results were obtained:—

Sulphuric ether	11 ⁰
Formic ether	12
Acetic ether	15
Olefiant gas	39

The verification of the above theoretic reasoning is here complete. It is proved that *in a long tube the dynamic radiation of the vapour exceeds that of olefiant gas, while in a short tube the dynamic radiation of the gas far exceeds that of the vapour.*

§ 8. The apparatus with which these experiments were made is capable of very diverse uses. Attached to a compression pump, with it the relation between the mechanical force expended in compressing a gas and the heat developed might be accurately determined. If oxygen, hydrogen, nitrogen, or air were the body compressed, the conversion of *vis viva* into heat might be declared by a modicum of vapour always kept in the tube, while a compound gas would tell its own tale.

Another interesting point might be, and indeed has been, settled by the apparatus. Some years ago a discussion was carried on between Professors Challis and Stokes on Laplace's correction for the velocity of sound, Professor Challis contending

that Laplace had no right to his correction, inasmuch as the heat developed by the local compression of a mass of air of indefinite extension would be instantly wasted by radiation. Experiments, he argued, conducted in confined vessels furnish no ground for drawing conclusions regarding what occurs in the atmosphere, where the heat developed has an indefinite space to lose itself. In our experimental tube, though it is mechanically closed, indefinite extension, as regards the radiation of heat, is secured in one direction, and the means also exist of measuring the flux of heat in this direction. What is true for one direction is of course true for all, so that the apparatus will inform us of what must occur in the open atmosphere. Now, with the most powerful radiating gases which I have examined, the radiation continues a very sensible time, while the heat acquired by air on entering the tube is often a source of inconvenience on account of the inability of the air to disperse its heat by radiation. The question is therefore experimentally decided in favour of Laplace and his supporter.

I would here dwell for a moment on this comparative absence of radiating power on the part of air, and of the elementary gases generally. The air is the sole source of the heat which has warmed the vapours in our experiments on dynamic radiation ; it is related to them precisely as a hot polished metal plate is to the coat of varnish which makes it a radiator. The air and the metal (both elements or mixtures of elements) are incompetent to impart motion to the luminiferous ether without the intermediation of a second body. They possess the motion, but they are so related to the ether that they cannot communicate this motion to it *directly*, or only in an extremely feeble degree. The atoms of air oscillate, but the ether does not swell. We have here a definite picture before the mind's eye, which, if the theory of an ether be true, is as certain as any conclusion of mathematics, and would hardly be rendered more certain if the physical vision were so sharpened as to be able to see the oscillating atom and the fluid in which it swings. I write thus strongly and definitely lest it should be imagined that I am dealing in vague conjectures in connexion with this subject. If I am vague, the mechanical theory of an ether must in reality bear the reproach. So far, however, from having a reproach to bear, the whole body of facts is in complete harmony with this theory.

Further, if, as all the facts declare, radiation and absorption are complementary acts, the one consisting in communication, the other in reception, and the one being strictly proportional to the other, no coincidence in period between the vibrations of a radiating body and those of oxygen, hydrogen, or air could make any one of these substances a good absorber. The form

of the atom, or some other attribute than its period of oscillation, must enter into the question of absorption. It is physically incapacitated from communicating motion, and hence in an equal degree incapacitated from accepting motion. The neutrality of elementary gases in the experiments on absorption already recorded does not arise from my accidentally choosing a source of heat whose periods do not synchronize with those of the gas; for however they might synchronize, the gas would still be a bad absorber. Even when the motion which their own absorbent power does not enable them to take up is mechanically imparted, or is communicated to them by contact, they are still incompetent to expend it upon the ether, which accepts all vibrations alike*.

§ 9. Scents and effluvia generally have long excited the attention of observant men, and they have formed favourite illustrations regarding the divisibility of matter. Several chapters in the works of the celebrated Robert Boyle are devoted to this subject, and eminent men in all countries have speculated more or less upon the extraordinary tenuity of the matter which is competent to produce sensible effects upon our organs of smell. Such a subject would of course in itself form a wide inquiry, which it is quite out of my power to develope at present. I think, however, that the apparatus which we have thus far made use of enables us to deal with the question in a manner hitherto unattainable.

A number of dry aromatic plants† were obtained from Covent Garden, the leaves and flowers of which were stuffed into glass tubes 18 inches long and a quarter of an inch in diameter. By means of my second air-pump, a current of dry air was first passed over them for some minutes. They were then connected with the experimental tube, which had its sources of heat arranged as already described. The tube was first exhausted and the needle brought to 0°, and dry air was then passed over the scented herbs until the tube was filled. The consequent deflection was noted, and from it the absorbent action of the odorous substance was deduced.

Thyme thus treated exercised thirty-three times the absorption of the air in which it was diffused.

* I can hardly imagine the bands in the spectra of metallic compounds to be produced by the vibration of the compound atom. All my experiments show the vast influence of chemical union on the rate of oscillation; the metal itself and the compound of that metal could hardly, in my opinion, oscillate alike. Hence I infer that decomposition has occurred when the bright and constant spectral bands are seen.—June 20th.

† I mean "dry" in the common acceptation of the term. They were not green, but withered; doubtless, strictly speaking, they contained aqueous vapour.

Phil. Mag. S. 4. Vol. 24. No. 162. Nov. 1862. 2 A

Peppermint exercised thirty-four times the action of the air.

Spearmint exercised thirty-eight times the same action.

Lavender produced thirty-two times the action of the air.

Wormwood forty-one times the action of the air.

The following perfumes were obtained from Mr. Atkinson of Bond Street, and examined in this manner. Small squares of dried bibulous paper, all of the same size, were rolled into cylinders about 2 inches in length; each of these was moistened by an aromatic oil, and introduced into a glass tube between the drying-apparatus and the experimental tube. The latter being first exhausted, was afterwards filled by a current of dry air which had passed over the scented paper. Calling the action of the air which formed the vehicle of the perfumes 1, the following absorptions were observed in the respective cases:—

TABLE VII.

Name of perfume.	Absorption.	Name of perfume.	Absorption.
Pachouli	30	Lavender	60
Sandal Wood . . .	32	Lemon	65
Geranium	33	Portugal	67
Oil of Cloves . . .	33·5	Thyme	68
Otto of Roses . . .	36·5	Rosemary	74
Bergamot	44	Oil of Laurel	80
Neroli	47	Cassia	109

It would be interesting to examine the absolute weights of the substances which produced these effects; but this I suppose is a task which chemistry is unable to accomplish. In comparison with the air which carried the odours into the tube, their weight must be almost infinitely small. Still we find that the least energetic in the list has thirty times the effect of the air, while the most energetic produces 109 times the same effect. As regards the absorption of radiant heat, the perfume of a flower-bed may be more efficacious than the entire oxygen and nitrogen of the atmosphere above it.

After each scent had been introduced, a stream of dry air was admitted at one end of the tube, while the pump was worked in connexion with the other. The perfume was thus cleared out until the needle returned to 0°. This was often a long operation, the odours clung with such tenacity to the apparatus. After the zero had been attained in the case of a strong perfume, a few minutes' rest of the pump sufficed to bring the scent from its hiding-places in the crevices and cocks of the apparatus, and almost to restore the original deflection. The quantity of those residues must be left to the imagination to conceive. If they were multiplied by billions they probably would not reach the density of the air.

Fearing that the more active perfumes might possibly prejudice the action of the more feeble ones which succeeded them, I made a series of experiments with the following essences, and obtained the results recorded :—

Camomile flowers	87
Spikenard	355
Aniseed	372

After this enormous effect I repeated the experiment with bergamot, and found its action to be exactly the same as that recorded in the Table.

I made a few experiments on musk, but obtained different results with it at different times. On the 16th of October I obtained some fresh musk from the perfumer's, placed it in a small glass tube, and carried dry air over it into the experimental tube. The first experiment gave me an absorption of 74, the air which carried the perfume being unity. A second experiment, in which the air was admitted more quickly, the absorption was 72.

It would be idle to speculate upon the quantity of matter which produced this result. The stories regarding the unwasting character of this substance are well known; suffice it to say that a quantity of its odour carried into the tube by a current of air of a minute's duration, produced an effect seventy-two times that of the air which carried it. Long-continued pumping failed to cleanse the tube and passages of the musk. It cannot be volatile, for an amount of ether vapour which produces a far greater action is speedily cleared away, while the cocks and connecting-pieces of the air-pump had to be boiled in a solution of soda before they were fit for use after the experiments with this substance.

Two perfectly concurrent experiments with ordinary cinnamon, in which fragments of the substance were placed in a tube and had dry air passed over them, gave an absorption of 53.

Several kinds of tea, treated in the same manner, produced absorptions which varied between 20 and 28.

In the teas, cinnamon, musk, and the odorous plants already referred to, dry air had been passed over them for some time before they were examined. Still a small amount of aqueous vapour may have entered with the odours, and thus rendered the results to some extent of a mixed character.

§ 10. *Ozone*.—In my last memoir I alluded briefly to the action of ozone; but the experiments then made having been executed with a brass tube, I was very desirous of repeating them with a tube which could not be attacked by this extraordinary substance. Experiments with the glass tube, performed on the 16th, 17th,

and 18th of last July, satisfied me that I had not over-estimated its power as an absorber of radiant heat.

In my first experiments I made use of large electrodes, for the purpose of lessening the resistance to the passage of the current through the decomposing liquid. The oxygen thus obtained differed but little from ordinary oxygen.

This year I had three decomposing-vessels constructed: in the first (No. 1) the platinum plates had about four square inches of surface, being rolled up to economize space; the plates of the second (No. 2) had two square inches of surface, while those of the third (No. 3) had only a square inch of surface each. Numerous experiments with these gave me the following constant results. Calling the absorption of ordinary oxygen 1,—

Electrolytic Oxygen.

From plates.	Absorption.
No. 1	20
No. 2	34
No. 3	47

A series of experiments made on the following day gave these results:—

No. I.	21
No. II.	36
No. III.	47

I now cut away a portion of the plates of No. II. so as to make them smaller than those of No. III. The oxygen obtained with these plates gave an absorption of 65, thus exceeding No. III. considerably. The plates of No. III. were now reduced so as to make them smallest of all; the oxygen which they delivered gave an absorption of 85. I feared the development of heat with these smallest plates, and, knowing heat to be very destructive of ozone, I surrounded the apparatus by a mixture of pounded ice and salt. The absorption of the oxygen thus obtained with the smallest plates amounted to 136.

By the results already recorded we have been prepared for the effect of minute quantities of matter; otherwise we could not fail to be struck with astonishment on finding a quantity of this substance which would elude all attempts on the part of the chemist to determine its amount, producing an effect so stupendous in comparison with common oxygen. I have, moreover, strong reason to believe that I understate considerably the effect of the ozone. The experiments exhibit in an extremely striking manner the great influence of the density of the current at the place where the oxygen is liberated on the amount of ozone developed.

§ 11. All the results here recorded had been obtained, when, turning to De la Rive's excellent treatise on Electricity, I found an allusion to the experiments of M. Meidinger on ozone. I had never previously heard any allusion made to this investigation, and was gratified to find in it the record of a very interesting piece of work.

M. Meidinger commences by showing the absence of agreement between theory and experiment in the decomposition of water, the difference showing itself very decidedly in a deficiency of oxygen *when the current was strong*. On heating his electrolyte, he found that this difference disappeared, the proper quantity of oxygen being liberated. He at once surmised that the defect of oxygen might be due to the formation of ozone; but in what way, was still to be determined. If it were due to the great density of ozone in the tube which received the oxygen, the destruction of this substance by heat would restore the oxygen to its true volume. Strong heating, however, which destroyed the ozone, showed in repeated measurements no alteration of volume, hence M. Meidinger concluded that the defect which he had observed was not due to the ozone mixed with the oxygen itself. He finally concluded, and justified his conclusion by satisfactory experiments, that the loss of oxygen was caused by the formation of peroxide of hydrogen, which was dissolved in the liquid and thus withdrawn from the electrolytic gas. He was further led to experiment with electrodes of different sizes, and found the loss of oxygen much more considerable when a small electrode was used than with a large one, whence he inferred that the formation of ozone was facilitated by *augmenting the density of the current at the place where electrode and electrolyte meet*. Nothing could be more different from the method pursued by M. Meidinger than that by which I arrived at the same conclusion; and though I had no doubt of the accuracy of my experiments, it was pleasant to find them corroborated in such a remarkable and unexpected way. I may add that since the perusal of M. Meidinger's paper I have repeated his experiments with my decomposition-cells, and find that those which gave me the greatest absorption also show the greatest deficiency in the amount of oxygen liberated*.

The quantities of ozone with which I have operated must be perfectly unmeasurable by ordinary means. The action of the substance is like that of olefiant gas, or boracic ether; bulk for bulk it might transcend either. No elementary gas that I have examined behaves at all like ozone. In its swing through the ether it must powerfully disturb the medium. If it be oxygen, it must

* I have recently learned that M. de la Rive was the first to observe the influence of the size of the electrodes on the development of ozone.

be oxygen packed into groups of atoms, which encounter vast resistance in moving through the ether. I sought to decide the question whether it is oxygen or a compound of hydrogen, in the following way. Heat destroys ozone. If it were oxygen only, heat would convert it into the common gas; if it were the hydrogen compound which some chemists consider it to be, heat would convert it into oxygen plus aqueous vapour. The gas alone admitted into my tube would give the neutral action of oxygen, but the gas plus the aqueous vapour I hoped might give a sensibly greater action. I caused the dry electrolytic gas to pass through a glass tube heated to redness direct into the experimental tube. I afterwards introduced a drying-tube between the place where the gas was heated and the experimental tube. Hitherto I have not been able to establish with certainty a difference between the dried and undried gas. Previously to heating, the electrolytic oxygen had been scrupulously dried; if the act of heating developed aqueous vapour, I can only say that the experimental means which I have employed are unable to detect it. For the present, therefore, I hold the belief that ozone is produced by the packing of the atoms of elementary oxygen into oscillating groups—and that heating dissolves the bond of union and allows the atoms to swing singly, thus disqualifying them for either intercepting or generating the motion which as systems they were competent to intercept and generate.

[To be continued.]

XLVI. *Dalton's Theory of Vapour, and its Application to the Aqueous Vapour of the Atmosphere.* (Extract of a Letter from Professor LAMONT to Professor KÄMTZ at Dorpat, dated Munich, August 28, 1862.)

To the Editors of the Philosophical Magazine and Journal.

Royal Observatory, Greenwich,
October 11, 1862.

GENTLEMEN,

THE paper, of which I enclose a translation, has been circulated in a printed form by Dr. Lamont. The importance of the subject in reference to all meteorological inquiries, especially to those on the broad scale, may, I trust, be accepted as my excuse for submitting it for publication in the *Philosophical Magazine*.

As far as observations have enabled me to form an opinion, I assent entirely to the views of Dr. Lamont.

For the translation itself, I am indebted to the friendly assistance of W. T. Lynn, Esq., Assistant of the Royal Observatory.

I am, Gentlemen,

Your obedient Servant,

G. B. AIRY.

You will, I believe, agree with me in this, that we are now arrived at that point in meteorology where it is absolutely necessary to come to a definite decision as to what is the relation in which the aqueous vapour existing in the atmosphere stands to the atmosphere itself. Does the aqueous vapour form an atmosphere itself independent of the air, or is it merely mechanically mixed with the air, so as only, as a gas standing in no chemical relation to the air, to increase the volume and the weight of the atmosphere?

Of the many important questions which have reference to the variations of the barometer, none can be thoroughly investigated without first coming to a clear understanding of this. At the same time the matter here in hand is an important problem of general physics, in regard to which also the mutual relations of air and vapour have hitherto been by no means ascertained with the necessary certainty. An investigation which I have instituted in this direction has now, it appears to me, led to a decisive result; and I believe that I ought so much the more to make communication to you upon it, as the result obtained is contradictory to the generally prevailing views of physical philosophers and meteorologists, and leads to the necessity of in part supplanting the principles which have hitherto had universal acceptance, in regard to aqueous vapour, by new ones.

On account of the connexion, it will be in the first place necessary that I should notice the progress of the development of the theory of aqueous vapour.

We have to consider Dalton as the originator of the theory of the action of aqueous vapour, he having instituted experiments so comprehensive and well contrived that nothing of importance has been added by the labours of later investigators. An accurate survey of Dalton's experiments will enable us to deduce from them the following principal results:—

(1) In space destitute of air, the evaporation of water goes on only until the vapour has attained a determinate expansive force, dependent on the temperature: so that in every space void of air which is saturated with vapour, a determinate vapour-pressure corresponds to a determinate temperature.

(2) In space filled with air, the same amount of water evaporates as in space destitute of air: and precisely the same relation subsists between the temperature and the expansive force, whether the space contains air or not.

(3) The evaporation of water goes on rapidly in space void of air, but very slowly in space filled with air in a state of quiescence: and even when it is assisted by a tolerably brisk motion of the air, a considerable time is notwithstanding always required.

In this way are the development and extent of aqueous vapour

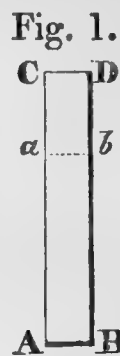
in space void of air and space filled with air determined by means of Dalton's experiments: as to the mutual relations subsisting between vapour and air, when they are simultaneously present in the same space, the experiments afford no information; and this deficiency Dalton supplied by giving to the second of the above-quoted propositions such an interpretation as if no mutual relation whatever existed between vapour and air, and as if they remained near each other without producing any the slightest mechanical effect upon one another. It is strange that physical philosophers have in general inconsiderately accepted this theory, so important and so pregnant with consequences, without remarking that it constitutes only a possible, but not a necessary result of the experiments. Not less singular is it that meteorologists have treated Dalton's theory as available for application to the aqueous vapour of the atmosphere, and have supposed an atmosphere of vapour to exist independent of the air, and sustained in equilibrium by itself alone, notwithstanding that the third of the above propositions properly declares that there is indeed always a tendency to a normal relation, which is conceived to be in the state of restoration, but which is never reached, because, in consequence of the changes constantly taking place, the requisite time to produce an equalization is never afforded.

Objections have from time to time been brought forward to the existence of an atmosphere of vapour independently subsisting. Bessel has (*Ast. Nach.* No. 236) adduced the consideration that in such a vapour-atmosphere the expansive force of the strata incumbent upon one another must diminish according to a determinate proportion, but that, from different observations, it may be concluded that this proportion does not really exist; his arguments, however, appear—principally, perhaps, owing to the want of sufficient data from observation—to have produced no impression; nor was more notice taken of the experiments of Broun in Makerstoun (Report to Sir T. Brisbane) and Jelinek in Prague (*Denkschriften der Wiener Akad. math.-naturw. Classe*, vol. ii.), who proved by experiments that in different localities situated very near together, where the same reading of the barometer is observed, a very different vapour-pressure may be indicated. One of the most zealous opponents of Dalton's theory was Espy, who (especially in his second Report on Meteorology) exposed its defects with much penetration, without, however, furnishing a precise refutation. I believe that I have myself brought forward the first proof of the incorrectness of the theory (*Denkschriften der Münchner Akad. math.-phys. Classe*, vol. viii.) in the year 1857, when I showed, by means of observations extending through many years, that in a small

vapour-pressure the mean reading of the barometer stands quite as high as in a great vapour-pressure; at the same time I contrived an easily-performed experiment, in which, contradictory to Dalton's theory, a mass of vapour and a mass of air, placed in communication with each other, mutually preserve a state of equilibrium without the vapour penetrating into the air or the air into the vapour. As the result of this, I laid down the proposition that the vapour exerts a pressure upon the air and the air upon the vapour; and the atmosphere is to be regarded as a mixture of masses of air, some more and some less humid. Strachey furnished a second very solid proof of the inadmissibility of Dalton's theory in a paper which he read before the Royal Society of London in the year 1861. Proceeding upon considerations which are fundamentally identical with those developed by Bessel, he gave a collection of the results of observations which had been obtained upon high mountains and in air-balloon expeditions, and showed that they were incompatible with the supposition of an independently-subsisting atmosphere of vapour. To instance one point only, it may be here mentioned that the observations of Welsh, who ascended in a balloon to the height of 23,000 feet, place us in a position to calculate the pressure which the vapour contained in the atmosphere would exert on the earth's surface; but the value determined in this manner amounts only to the fourth part of the pressure actually assigned by the psychrometer. It might have been supposed, from the clearness of the proofs adduced and the close agreement of all the results of observation, that a finally satisfactory decision would have been arrived at; nevertheless we find, even in the most recent times, that the "pressure of the dry air" and the "pressure of the vapour-atmosphere" are, as before, kept distinct the one from the other. There is, I believe, no other means of removing the rooted ideas in consequence of which "*Dalton's laws*" are constantly appealed to, than the direct proof that *Dalton's laws themselves contain an essential error*.

With this view I undertook a short time since the series of experiments to which I referred at the outset. I first convinced myself how extremely slowly the vapour in the air spreads itself from one part of space to another, if, without destroying the communication between them, the free circulation of the air be restrained. It is mainly the circulation of the air that carries off the vapour from the evaporating surface, and conveys the vapour, when already diffused, to the chloride of calcium to be absorbed; one would almost believe that the individual molecules of air must come to the surface of the water to take thence the moisture, and to the chloride of calcium to give up to it the moisture;

the expansive force of the vapour itself is in every case a matter of small influence on its diffusion in the air. Now, if we take a closed tube, A B C D, filled with air, and introduce a small quantity of water through an aperture near A, which is afterwards immediately closed, into the bottom A B of the tube, the water begins gradually to evaporate, and the vapour ascends, after the expiration of a certain time, up to $a b$. How then will the pressure be distributed upon the interior sides of the tube?



If, as I have endeavoured to prove, by means of the above-mentioned experiment, the vapour and the air exert a mutual pressure upon one another, the expansive forces of the air and of the vapour will act together in such a manner that an amount equal to *their sum* will press upon all points of the interior wall; and if we take separately the pressure peculiar to the vapour alone, it is precisely as great as if the mass of vapour was uniformly distributed in the whole space A B C D. A totally different state of things will result if the view set up by Dalton, and generally accepted by philosophers, is well founded; for as, according to this view, the vapour diffuses itself in the interstices of the molecules of air, *without producing any mechanical effect whatever upon the molecules themselves*, no pressure at all can be produced upon the interior side of the tube by the expansive force of the vapour, under the circumstances indicated above; and no pressure takes place until the vapour reaches the upper surface C D.

The state of things here indicated is only a *transitory* one; a similar state may, however, be made *permanent* by maintaining in the lower space A B $a b$ a higher, and in the upper space $a b$ C D a lower temperature. If we denote the lower space by V , the upper by V' , the lower temperature by t , the higher by t' , and the corresponding expansive forces of the vapour by $f(t)$ and $f(t')$; also the expansive forces of the enclosed masses of air by $k(1 + \alpha t)$ and $k(1 + \alpha t')$, we have, according to the hypothesis advocated by me, the expansive force of the mixture

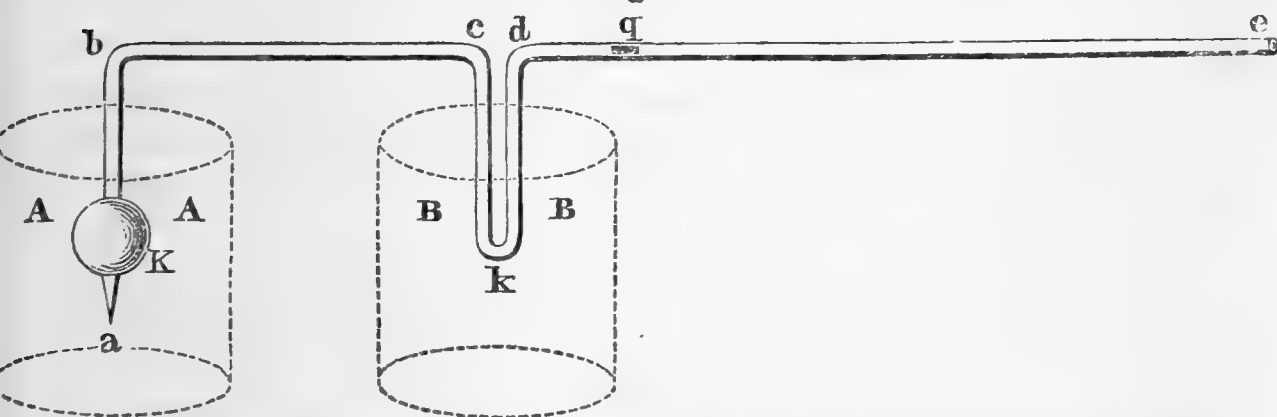
$$\begin{aligned} &= \frac{V}{V + V'} [k(1 + \alpha t) + f(t)] + \frac{V'}{V + V'} [k(1 + \alpha t') + f(t')] \\ &= k + \frac{k\alpha}{V + V'} (Vt + V't') + \frac{1}{V + V'} (Vf(t) + V'f(t')), \end{aligned}$$

whereas, according to Dalton's theory, the expansive force will only amount to

$$k + \frac{k\alpha}{V + V'} (Vt + V't') + f(t'),$$

while the vapour passing into the space $abCD$ with the force $f(t) - f(t')$ must be immediately condensed. Thence it immediately follows that, if the temperature t' of the upper space continues constant while the temperature of the lower space gradually increases, the pressure upon the upper surface CD is increased, according to Dalton's theory, only by the expansion of the air, but not by the newly-forming vapour itself, whereas, according to my hypothesis, besides the effect which is produced by the expansion of the air, a very considerable augmentation of the pressure arises from the newly-formed vapour. As the conditions here indicated admit of being practically placed in operation, we have a simple and certain means of coming to a decision regarding the correctness of Dalton's theory; and the only thing requisite is to arrange an appropriate contrivance for the experiment. I have selected the following: A glass tube bent in the form represented in fig. 2 was provided at one end with a globe K , whilst the other end e was left open; in the straight part de , it was made to contain a drop of quicksilver, q . The curved part ckd of the tube was plunged into a vessel, BB , filled with cold water; into the vessel AA , where the

Fig. 2.



globe K was placed, cold and warm water could in turn be poured. The globe K was first filled with dry air; and the experiment showed that if the temperature was increased from $15^{\circ}7$ to $41^{\circ}8$, the drop of quicksilver moved forwards by 11.47 Paris inches.

During this experiment, a thermometer placed in the vessel BB stood at 12° . Afterwards the globe was opened by breaking off the fine point a , some water introduced, and the point again joined on by melting. Again cold and warm water were poured into the vessel AA whilst the temperature of the tube remained unaltered, by which means, according to the theory of Dalton, a rise of temperature from $15^{\circ}7$ to $41^{\circ}8$ would, if the vapour had not penetrated in the tube up to the drop of quicksilver, move the latter as before 11.47 inches, and if the vapour had so penetrated, at the most $\frac{1}{58}$ th further; instead

of which, the motion actually produced amounted to nearly the double of this. It resulted from accurate measurement, that the 11·47 inches were passed over as soon as the temperature had been raised from $15^{\circ}\cdot7$ to $30^{\circ}\cdot9$.

A second glass tube was employed of a similar form, but with a smaller globe; and with this, as long there was only dry air in the globe, an increase of temperature of from $14^{\circ}\cdot4$ to $44^{\circ}\cdot24$ occasioned in the drop of quicksilver a motion of 12·86 Paris inches; but after a small quantity of water had been introduced into the globe, the quicksilver moved the same distance when the temperature was raised only from $14^{\circ}\cdot4$ to $31^{\circ}\cdot1$. As it might be imagined that it was possible that, after a longer interval of time, the vapour would extend up to the drop of quicksilver and then produce a different result, the globe was left for a whole hour in warm water, but the position of the quicksilver remained unchanged.

Also, after the termination of the experiment, neither in the first nor in the second tube could a trace be perceived of the vapour having passed down into the bent part between *c* and *d*; so that it probably penetrated into the tubes either not at all or only to a small extent. On this supposition the observed effect would require the conclusion that the increase in the expansive force of the dry air in a change of temperature from $15^{\circ}\cdot7$ to $41^{\circ}\cdot8$ is precisely as great as the increase in the expansive force of the air and the aqueous vapour in a change of temperature from $15^{\circ}\cdot7$ to $30^{\circ}\cdot9$; and this also agrees exactly, for the former increase is calculated to be 0·119, and the latter amounts to—

For the air	0·070
For the vapour	0·048
Therefore, together . . .	<u>0·118</u>

In the second experiment we have the increase of the expansive force

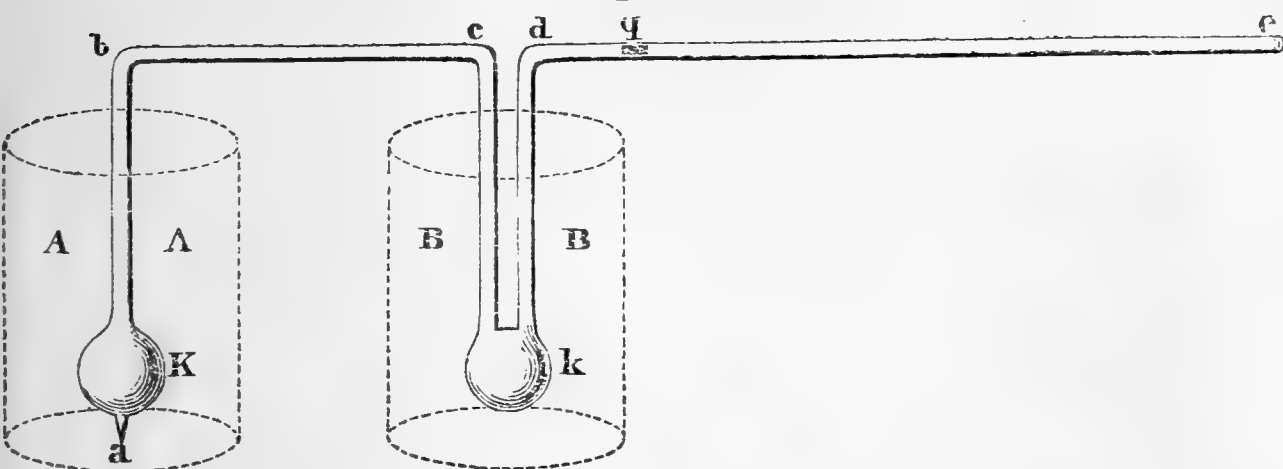
For dry air, from $14^{\circ}\cdot4$ to $44^{\circ}\cdot24$	0·136
Then	
For air, from $14^{\circ}\cdot4$ to $31^{\circ}\cdot1$	0·076
For vapour, from $14^{\circ}\cdot4$ to $31^{\circ}\cdot1$	0·052
Therefore, together	<u>0·128</u>

little differing from the preceding number.

In order to obtain still greater certainty, I modified the experiment in the following manner:—I gave the glass tube the form represented in fig. 3, which differs from the form previously used in this particular, that at *k* a globe is attached of about the same size as the globe *K*; moreover the vessel *B B* was filled with pounded ice and water, so that the temperature was constantly maintained at $+0^{\circ}\cdot2$. The results were as follows:—

(1) When the tube was filled with dry air, the drop of quicksilver moved, during a rise of temperature from $12^{\circ}\cdot7$ to $49^{\circ}\cdot2$ a distance of 11·38 Paris inches.

Fig. 3.



(2) When some water was introduced into the globe K, the quicksilver moved the same distance of 11·38 Paris inches during a rise of temperature of only from $12^{\circ}\cdot7$ to $35^{\circ}\cdot3$.

If we calculate in the same manner as above the increase of expansive force, we obtain—

For dry air with a rise of temperature from $12^{\circ}\cdot7$ to $49^{\circ}\cdot2$ 0·174

For air and vapour together, with a rise of temperature from $12^{\circ}\cdot7$ to $35^{\circ}\cdot3$, separately,

For air 0·103

For vapour 0·082

Together } 0·185

This result is somewhat greater than that obtained for dry air; ground is therefore afforded for conjecturing that some vapour must have passed from the globe K towards k; the quantity, however, can only have been very small; for when the vessel A A contained water of constant temperature (13°) and cold and warm water alternately were introduced into the vessel B B, in order to move the quicksilver 7·80 Paris inches a rise of temperature was requisite as follows:—

Before the above experiments . . . from $13^{\circ}\cdot5$ to $47^{\circ}\cdot0$

After the above experiments . . . from $13^{\circ}\cdot5$ to $45^{\circ}\cdot8$

and afterwards, when the globe K had stood for two hours together in water at a temperature from 35° to 40° ,

from $13^{\circ}\cdot5$ to $44^{\circ}\cdot6$.

From the latter determination it is to be inferred that, notwithstanding a considerable vapour-pressure was maintained for so long a time in the globe K, yet not so much vapour had passed into the globe k as would have been requisite for the

saturation of the space at a temperature of 0° , although the aperture of the tube had a diameter of 1.1 Paris lines.

We are incontestably entitled to conclude from these experiments that Dalton's theory, in so far as it assumes that the air and the vapour existing in the same space are independent of each other, is totally unfounded; the true view rather is that *the air exerts a pressure upon the vapour and the vapour upon the air*. I make use here of this mode of expression merely in order to represent the effect. I hope at a future opportunity to be able to show that the humidity must be regarded as adhering to the molecules of air, and that the phenomena admit of a simple explanation by means of a natural hypothesis concerning the expansion of dry and wet molecules of air.

If it be desired to apply the theory developed in the foregoing to the circumstances of the aqueous vapour in the atmosphere, it is in the first place to be inferred from it that (since the diffusion of the vapour in the air takes place but very slowly, and since in different places, according to the temperature and the magnitude of the surface of the water exposed to the air, very different quantities of vapour pass into it) in regard to the humidity of the air, strictly speaking, no relations subsist conformable to any law. Of course the continually existing currents of air occasion a complete mingling of the more and less dry masses of air; but this takes place in no uniform manner, and therefore no exact relation of dependence exists between the degrees of humidity in different points of any space. But particularly the idea of an atmosphere of vapour subsisting independently by itself appears to be inadmissible, and the data furnished by the psychrometer can no longer be regarded in any other light than as the expression of *local humidity*.

XLVII. *On the Motion of Camphor towards the Light.* By CHARLES TOMLINSON, *Lecturer on Physical Science, King's College School, London**.

IN Chaptal's *Elémens de Chimie* (vol. i. p. 36), published in 1790, a property is claimed for light which has been recognized by later philosophers. For example, in Professor Daniell's 'Introduction to the Study of Chemical Philosophy' (2nd ed., 1843, p. 452), we read, "Light is capable of acting upon and directing homogeneous attraction, and of influencing the crystallization of certain substances. Evidence of this may very commonly be found in druggists' shops, where, in the glass jars which contain camphor and are placed in the windows, beautiful crystals

* Communicated by the Author, having been read at the British Association at Cambridge, October 1862.

may generally be seen attached to the sides the nearest to the light. Many other substances which are capable of the same kind of sublimation exhibit the same phenomena in their solidification.

“M. Chaptal first made the observation that when a number of capillary crystals shoot up the sides of a glass vessel containing a saline solution, they attach themselves only to that side of the vessel which is most strongly illuminated. He was thus able to cause crystals to form on any selected side; and by placing a screen before the vessel, he found that the line between light and darkness was distinctly marked by the limit of crystallization. This result is most readily obtained with the metallic salts.”

Professor Brande, in his ‘Manual of Chemistry’ (1848, p. 11), has a similar statement, and our latest printed authority, Professor Miller’s ‘Elements of Chemistry’ (vol. iii. 2nd edit., 1862), states (p. 540) that camphor “becomes slowly volatilized at common temperatures; and if kept in glass bottles, is gradually sublimed and condensed in octahedral crystals on the side of the vessel which is exposed to the light.”

The motion of camphor towards the light was investigated by Dr. Draper, Professor of Chemistry in New York, who, in the Appendix to a work published in 1844, entitled “Treatise on the Forces which produce the organization of Plants,” brings together the results of his experiments on the camphor-motions which had been previously published in scientific journals.

It did not escape the notice of the early observers that other substances besides camphor moved towards the light in the glass vessels that contained them. Thus M. Dorthes, one of Chaptal’s laboratory pupils, states that spirits of wine, water, &c., “always condense on the most illuminated sides of the vessels.” Dr. Draper found that if iodine be heated in a glass vessel and be placed in the sun, the vapour would in like manner condense on the most illuminated side. His method of operating with camphor was to place it in a vessel, which was then exhausted of air, and so moved into the sun. A crystalline deposit would be made on the sunny side in about five minutes, and this would increase during the next two hours, and sometimes cover the whole side of the glass. If a ring of tinfoil $1\frac{1}{2}$ inch internal diameter and $\frac{1}{2}$ an inch wide were attached to the glass, it prevented the deposit in and about it, and would even remove a deposit already formed. A similar effect was produced if the ring were placed near the glass instead of being in contact with it.

Dr. Draper states, further, that the camphor-vessel may be kept in the dark for any length of time without producing a

deposit; that artificial light has no effect in producing a deposit; that the deposits are sometimes furthest from the sun, and at other times nearest: these are termed *aphelion* and *perihelion* movements. Dr. Draper says, "The sun's rays have the power of causing vapours to pass to the perihelion side of vessels in which they are confined; but, as it would appear, not at all seasons of the year. For example, I have a certain glass fitted up for making these observations; and in this vessel, during December, January, and part of February 1836-37, a deposit was uniformly made towards the sun; during March, April, and May next following, although every part of the arrangement remained to all appearance the same, yet the camphor was deposited on the side furthest from the sun. It does not appear that any immediate cause can be assigned for this waywardness."

It was further found that when the sun's light was passed through water and solutions of ammonio-sulphate of copper and of bichromate of potash, the crystallization was on the *aphelion* side.

The following statement is also made:—"Light which has suffered reflexion at certain angles seems to have undergone a remarkable modification, being no longer able to put the glass into such a condition that it can cause motion towards the sun. Under such circumstances crystallization proceeds with rapidity, not on the perihelion side of the vessel, but on the opposite side. This result is not supposed to be due to polarization, as it takes place at all angles."

A very few words will explain Dr. Draper's theoretical views. From the results of an experiment made with a differential thermometer, he "cannot admit that the rays of heat have any active part in bringing about the phenomena." He was strongly inclined to the opinion, from the action of the tinfoil rings, that the motion of camphor to the light was an electrical phenomenon; but he failed to detect electricity, though sought for by means of a delicate electrometer. His ultimate conclusion seems to have been that these deposits are due to a mechanical action of light. In short, the result of Dr. Draper's elaborate inquiry was to multiply phenomena and to leave the theory as it was.

I am not aware that this subject has been investigated by any one since the publication of Dr. Draper's volume. In repeating his experiments, I found it necessary to get rid of the air-pump and of exhausted vessels altogether, in order to be at liberty to multiply experiments to any extent. In some comparative trials it was found that raw camphor was more sensitive than refined, and it was accordingly adopted. Common corked and stoppled bottles of white and coloured glass were used, varying in capa-

city from a few ounces to a quart and more. These were charged with various quantities of crude camphor, from 2 or 3 grains to 100, and were placed in the window on flat supports at the junction of the sash-bars, and also in various other positions with respect to the light as occasion required. An arrow pointing to the light was marked on every bottle, and in many cases the date when the bottle was first exposed.

It may here be stated once for all, that the substances which produce good deposits are ordinary refined camphor, Borneo camphor, oil of camphor, artificial turpentine camphor, chloral, naphthaline, iodine, mercury, sesquichloride of carbon, water, alcohol, &c.

I must confess that in the early part of the inquiry the whole subject of these camphor motions seemed to be beset with difficulties. I not only obtained Dr. Draper's results, but others equally surprising and contradictory so long as the action of light was looked for. I did not presume to suppose that Chaptal's statement, indorsed as it was by the honoured name of the late Professor Daniell, and by the names of all chemists of repute, was altogether a mistake; but when I considered somewhat the history of science, and remembered that Chaptal wrote at a time when men had no correct views on many parts of the science to which I was led to refer these phenomena—when, for example, the present theories of radiant heat and of dew, of evaporation and condensation did not exist,—I was led to think that my presumption might be pardoned if I ventured to propound an entirely new theory as to the motions of camphor, &c. towards the light.

As my object is to make a short statement, I omit a number of details which led me to adopt a new theory. I also omit many subsidiary facts observed during the inquiry. What led me to suspect that light had nothing to do with the camphor deposits, was the fact that during about a week of fine weather early in May the bottles in the east windows showed deposits *furthest* from the light, while those in the west windows were *nearest* to the light. In order to test this result more closely, four quart stoppled bottles, A, B, C, D, were supplied each with 100 grains of crude camphor in coarse powder and placed as follows:—A in the west window, B outside the west window on the balcony, C in the east window, and D outside the east window. The outside bottles were tied over with india-rubber cloth. In the course of the day deposits were obtained in all four bottles, varying in position and character, clearly establishing the fact that, while one of the indoor bottles was forming a deposit furthest from the light, the corresponding outside bottle had its deposit nearest to the light. The two bottles, within a few feet

of each other, gave contradictory evidence. They were both placed under the same circumstances as to light, but with this important difference as to weather—that one was protected from it, and the other exposed to it. In fact there was no escaping from the conclusion, notwithstanding Dr. Draper's protest, that heat is largely concerned in the production of the phenomena. It appeared in these early trials to be clearly made out that the vapour of camphor which filled the bottle was disposed of on the coldest surface of the glass, which might or might not be furthest from the source which supplied both the light and the heat, where it condensed after the manner of dew.

To test this idea, all that seemed necessary was to expose the vessel containing the camphor to the heating action of the sun apart from its light. Accordingly two cylindrical glass jars, A and B, 7 inches high and $2\frac{1}{2}$ inches in diameter, were charged with about 100 grains of crude camphor: their mouths were closed with bungs covered with tinfoil, and so enclosed in well-fitting canisters of tinned iron. A was placed in the west, and B in the east window, on the morning of the 8th of May. They were examined at 3 P.M., the temperature in the west window being 70° . A exhibited a faint deposit of crystals on the side of the glass furthest from the light, that is, on the coldest part of the jar. B gave no result, the sun having left the window before this canister was placed. The next morning B was again examined, and a faint deposit was found furthest from the light.

A result being thus obtained by heat alone, the experiment was varied in the following manner. Four 8-ounce phials, A, B, C, D, were charged, A with refined, and the others with crude camphor. A was covered with tinfoil, B was enclosed in a tin canister, C was covered with brown paper, and D was left naked. They were all arranged around a heated cannon-ball in a darkened room, at such distances as to be exposed to an initial temperature of nearly 90° . In less than half an hour C and D exhibited copious deposits of small crystals furthest from the source of heat. In an hour and a half A had a very faint furthest deposit, B no deposit.

I could now understand why the glasses in the tin canisters exposed to the sun exhibited only faint deposits. The canisters being good conductors of heat, made the glass of nearly the same temperature all round, and my theory required that one part should be colder than the other, and hence the faint deposit on the furthest side where it was only slightly colder; whereas brown paper being a bad conductor, would keep the glass warm on the side nearest the source of heat, and much colder on the furthest or opposite side, as was proved by the copiousness of the deposit.

But how did these results agree with Dr. Draper's assertion,

that reflected light produces furthest deposits? Does it? To test this, a camphor-bottle was furnished with a brown paper hood so as completely to cover it. A slit was then cut out $3\frac{1}{2}$ inches in length and $\frac{3}{4}$ inch in width, extending from near the top to the bottom of the bottle. A looking-glass was placed near to and facing an east window, and, a few inches off, the covered bottle with the slit opposite the looking-glass. The morning was wet and cloudy with occasional bursts of sunshine; but in the course of two hours a deposit was formed nearest the slit, and consequently nearest the reflected light. The experiment was repeated over and over again with the same result—modified in this way, that, if the morning sun were unusually hot, the deposit was scattered over a considerable portion of the interior of the bottle, but in such case the largest crystals were opposite the slit, where in fact the bottle was coldest. A bottle was wrapped up in tinfoil, and a slit only $\frac{1}{10}$ th of an inch wide cut out. The deposit was in this case confined to the slit and its vicinity. The results of these and many experiments satisfied me that the “remarkable modification” which Dr. Draper supposes reflected light to have undergone, whereby it produces only aphelion deposits, is really an effect of heat, capable of easy explanation.

The experiments on the protecting action of tinfoil rings, though not very original in their conception, produce admirable results. Had Dr. Draper been acquainted with Prevost’s experiments on dew*, and those of Carena on hoar frost†, he would have seen how tinfoil favours or prevents the deposition of moisture on glass according as its position favours or obstructs the radiation of heat. He would, moreover, have seen that his electrical theory is of no value in explaining phenomena which fall under the operation of those great laws which regulate the formation of dew.

It is true also that vapour of water and of iodine move towards the light, but only under certain circumstances. Dr. Draper’s method of performing the iodine experiment is, I think, characteristic of his general mode of inquiry. He raises the iodine in vapour, and then places the vessel containing it in the sunshine; that is, he hastens the result, and is satisfied when it is obtained. Had the iodine been exposed to the varying influences of heat and cold, sunshine and shade, it would have been found that the deposit, like that of camphor, and also of water, &c., is sometimes made towards the light, and at other times away from it. An experiment should speak with many tongues; and in order to enable it to do so, it must be repeated many times, under circum-

* *Annales de Chimie*, an xi. 1802–3.

† *Mémoires de l’Académie Royale des Sciences de Turin*, 1813–14.

stances sufficiently varied to detect the regulating law, and to eliminate disturbing causes.

In this examination of Dr. Draper's results, the inquiry became divested of some of its marvellous features, and pointed out the line of inquiry to be followed, and the theory to be established. Let it be granted that camphor, and other substances capable of being raised in vapour at ordinary temperatures, become, on the reduction of temperature, condensed on the coldest side of the vessel after the manner of dew, and all these varied phenomena range themselves in the most orderly manner under two well-known laws, namely, *radiation of heat*, whereby a surface, or a portion of a surface, becomes colder than the vapour in contact with it, and, secondly, *condensation of vapour* by the contact of the colder body. That is the theory which I have now to support.

In proceeding to apply this theory to Chaptal's experiments, I adopted certain variations in the mode of performing them; and naturally so, because, as the form of an experiment is but the expression of the thought that produces it, the one will vary with the other, as vessels cast in moulds of different patterns will vary. Chaptal exposed his solutions to the light in glass vessels, one-half of each vessel being covered with black taffeta, for the purpose, as he thought, of shutting out the light, when in fact he was merely preventing cooling by radiation. I obtain results identical with his in transparent vessels in the full sunshine by preventing radiation and evaporation in one-half of each vessel by covering it with a thin plate of glass or of mica. In such cases the exposed half of the vessel had a crystalline capillary deposit running round it, and increasing from day to day, and even passing over the edge of the glass and covering the outside, while on the covered portion there was no deposit whatever, or only a faint one after some days' exposure. Indeed it is not necessary that the covering glass plate should be in contact with the glass: if it only overshadow it, or be suspended over it, the preventive action is equally produced. I have had a solution of bichromate of potash in one vessel and a solution of sulphate of iron in a similar vessel at a lower level. The glass, which partly covered the first, projected over the second vessel without touching it; and in both cases, that is, in the covered half of one, and the overshadowed half of the other, there were no deposits, although there were abundant ones in the uncovered sides of both glasses.

Chaptal recognized the fact that when a large number of evaporating dishes were arranged according to his method in a small close room, no results were produced. He would have succeeded had he contrived some method of keeping the air dry. I put a solution of sulphate of copper, partly covered with a glass plate,

upon a shelf in a dark cupboard. No crystals were formed after many days, on account of the enclosed space becoming saturated with moisture, and thus preventing evaporation. To prove that the absence of light had nothing to do with the result, a capsule containing a few lumps of caustic potash was placed by the side of the solution, and the whole was covered with a bell-glass. In the course of a few hours the creeping crystals began to form on the side of the vessel not covered with the glass plate, just as in the case of similar solutions exposed to the light. Identical results were obtained when solutions of sulphate of zinc and of sulphate of copper were placed in the dark enclosed space without being covered with the bell-glass, provided the air were kept dry by means of caustic potash.

These experiments also serve to explain some facts which puzzled Dr. Draper. A bottle of camphor in the dark produces no deposit, even though left for months, but when put in the window, it forms a deposit immediately. One bottle put within another forms no deposit, even though exposed to the diffused light of day. Exactly so. A bottle shut up in the dark is protected from radiation: it is equally warm all round, and, though filled with vapour, there is no sufficient reason why a deposit should be made at one part of the bottle more than another. Put that bottle in the window, and it becomes cooled on the side nearest the window-pane, and a deposit is made. Light has nothing to do with this result. The experiment can be made in the dark as well as in the light; indeed much better, for by night there is a greater difference between the inner and the outer temperatures than by day. I have taken a bottle of camphor out of a dark cupboard, and placed it in the window on which the moon was shining. A deposit was made within three minutes on the side nearest the moon. Anyone satisfied with the result of one experiment, would say that the light of the moon produced the deposit. I have tried the same experiment when there was no moon, and obtained the same result. So also if a tube containing a little crude camphor be passed through a cork into the centre of an empty bottle, there will be no deposit on the tube on exposure to the light, because the tube is equally warmed all round. So also if a camphor bottle be surrounded with water at 100° or 110° and left to cool, there will be no deposit. In an experiment of this kind, a quart glass jar was three parts filled with water at about 110° , and an eight-ounce phial, containing crude camphor that had been kept in the dark some months without showing any signs of deposit, was plunged into it. The temperature was now 100° ; the jar was covered with a large air-pump receiver and left facing the light. Next morning the bottle was taken out and carefully examined; there

was no deposit whatever. The bottle was now wiped dry and put in the angle of the window on which the sun was shining. A thermometer placed near it marked 70° and rose to 78° . In less than 25 minutes there was a copious deposit of small crystals on the side nearest the sun. The warm woodwork prevented a deposit on the furthest side: in fact the deposit was formed on the side of the bottle where radiation was most free, even though that side was most exposed to the sun.

The result of this experiment was quite satisfactory to my mind; but as it is my duty to try and convince the minds of other men, I may be allowed to pile up a few of these proofs.

When the surface of the earth and the air resting upon it are of the same temperature, no dew is formed. If the earth be cooled ever so little, there is condensation: so also if the camphor bottle be surrounded by a medium of the same temperature, there is no deposit. Let that medium be of unequal temperature, and a deposit is immediately formed on the colder side. For example: a bottle containing crude camphor, which had been kept in a cupboard during some months without showing any sign of deposit (even when examined by that most delicate of all tests, a lighted candle), was taken out, and a circular piece of filtering paper of about the size of a florin was wetted with sulphuric ether, and so stuck on the outside of the bottle. In a few seconds an abundant deposit was made on the inside, exactly corresponding with the external plaister. The experiment was repeated with paper dipped in alcohol, bisulphide of carbon, &c., with the same result. I could now explain a result which had puzzled me not a little. Bottles containing crude camphor were from time to time placed on a shelf in a glazed bookcase close to the door and opposite a window, and were occasionally changed during several months. They were exposed to the diffused light of the room, and formed furthest deposits during March, April, and May, and no deposits at all during June, July, and August. Now, during the first three months the room had a fire in it; the wall formed the back of the bookcase, and glass doors its front; consequently the back part of the bottles would feel the stream of cold air from the door which passed along the wall to the fire, while the front of the bottles, protected by the glass and looking into the warm room, would preserve a higher temperature than the back; and hence deposits were formed at the back, and none in the front. When, however, fires were left off, the stream of cold air from the door would cease to flow, the inside of the bookcase would not vary in temperature, and the bottles, being protected from radiation, would not be in a condition to form deposits.

Whatever, then, protects the bottles from radiation, either wholly or in part, prevents the formation of deposits. The wooden scale of a thermometer hanging in a window a few inches from a bottle opposite the light, prevents a deposit in a broad line exactly corresponding with the form of the scale. A bottle placed near a bar of the window-frame will mark out the form of the bar by the camphor crystals coming up to within a certain distance of the bar, leaving that portion of the bottle naked which coincides with the form of the bar. In such a case as this, not only does the bar of wood prevent radiation from a portion of the bottle, but, by absorbing heat from the sun, it acts as a source of heat for hours afterwards. So also if a glass containing camphor stand on wood, the lower part is permanently warmer than the upper, as is shown by the repulsion of the deposit from the lower part, and that chiefly at the back—the front, where it is exposed to the light, being kept colder from being nearer the window. When bottles are tied over with flannel or india-rubber cloth, there is no deposit under these flaps. When glasses are closed with bungs, these retain the heat and keep the upper parts of vessels warmer than the middle parts, so that no deposit is made within half an inch or an inch of the bung. Coloured glass bottles produce furthest deposits, when exposed to the sun, more readily than white ones, because they sooner become heated on the exposed side.

The tinfoil rings of Dr. Draper act as screens in preventing radiation. The glass under and for a short space around the metal is kept warmer than the uncovered portion nearest the window, and hence no deposit in and about the protected portion. To prove that this is the correct explanation, it seemed to me that black absorbent substances would act more efficiently as protectors than bright reflecting ones. Accordingly I arranged a number of bottles containing crude camphor, and attached to them disks and rings of tinfoil, and of black, red, yellow, and white paper. The space kept clear by the black paper was many times larger than that cleared by the tinfoil and the lighter papers, and these all varied in extent of clearance with their colour. To show the protecting influence of white paper on a large scale, one half of a large cylindrical jar containing crude camphor was loosely covered with white paper in the direction of its length, and so placed in the window, the paper side nearest the light. There was an abundant deposit on the exposed half of the glass, which has remained some months, but no trace of a deposit has been found on the covered side.

I think enough has been stated to prove that the motion of camphor, &c. towards the light is really an effect of heat. The laws which regulate the deposit of dew and hoar frost apply here.

The bottles exposed in or near a window will always have one side colder than the other, and this colder surface will determine the deposit. Generally the side nearest the window is the coldest (seeing how little sun we have, and how long our nights are), and here the deposit is most copious; but when the sun shines on the window, and the side nearest the light is the hottest, a deposit is naturally made on the furthest side. This furthest deposit, however, is but transient. It disappears when the sun goes off the window, because the furthest side ceases to be the coldest. It goes, in fact, to augment the increasing deposit on the coldest side, or that nearest the light. I could always tell whether there had been any morning sun, by inspecting the east-staircase bottles on descending to breakfast. During the last spring and summer there would sometimes be sunshine and furthest deposits at 5 or 6 o'clock A.M., while clouds or rain would come on about 8, and the furthest deposits would disappear during the day.

It is scarcely necessary to prove that a large bottle placed in the window will be hottest on the side next the light when the sun is shining on the window, and coldest at other times, except perhaps during some of the warmest days of our short summer, when the external temperature is equal to, or even higher than the internal; but as I am supporting a new theory against the united testimony of many illustrious philosophers during three quarters of a century, it is scarcely possible for me to overstate my case. I will therefore give a few more details.

A glass shade with its mouth upwards was placed on a small table about 2 feet from a west window. Two thermometers which marked the same temperature were hung within it, one at the front or nearest the light, and the other at the back or furthest from it. The open mouth was then covered with a flat book. This was on the afternoon of the 7th of July, the weather being cloudy, with occasional bursts of sun. In half an hour the front thermometer read 82° , the back 78° . In another thirty minutes the weather was cloudy, windy, and threatening for rain; front 76° , back 74° . In another thirty minutes, during which heavy rain fell, the temperatures became inverted; the front was now 68° , and the back 74° . The colder temperature was on the side nearest the window during the night, and not until the sun had come round next day did the nearer side become the warmer.

Now if fresh bottles of camphor be placed near the window at intervals during all these mutations of temperature, it will be found that the deposits go to or from the light according as the front or the back of the bottle is the colder. There is no evidence of this fact more satisfactory than actual measurement; and to

be quite sure of my instruments, I asked Messrs. Negretti and Zambra to prepare two thermometers that should range together with considerable accuracy. These instruments were received during the very warm weather towards the end of July, and the results obtained by them struck me as remarkable. A cylindrical glass $10\frac{1}{2}$ inches high, and nearly 3 inches in diameter, was washed with sulphuric acid and an abundance of water, dried, and charged with crude camphor. The thermometers were suspended in the glass, front and back. The readings were taken at first every five minutes.

July 26, 1862. Sun on west window.

Remarks.	Furthest.	Time.	Nearest light.	Remarks.
	74° F.	5.40 P.M.	74° F.	
	83	5.45	83	Slight misty appearance in the glass.
	83 $\frac{1}{2}$	5.50	83 $\frac{3}{4}$	
	81	5.55	81 $\frac{1}{2}$	
	79	6.00	79 $\frac{1}{2}$	
	78	6.5	78 $\frac{1}{2}$	Clouds coming over the sun.
Faint camphor deposit. Moisture condensed high up on one side near the back.	77	6.10	77 $\frac{1}{2}$	Slightly clouded.
	72	6.15	72	
	72	6.20	72	
	76	6.25	76 $\frac{1}{2}$	
	77	6.30	76	Faint deposit.
	76 $\frac{1}{4}$	6.35	75 $\frac{3}{4}$	Do. increasing.
	76	6.40	75	Ditto.
	75 $\frac{1}{2}$	6.45	75	
	75	6.50	74 $\frac{1}{2}$	Deposit much more marked than on the other side, but still faint.
	75	6.55	74	
	74 $\frac{1}{2}$	7.00	73 $\frac{1}{2}$	
	73 $\frac{1}{2}$	7.10	72 $\frac{1}{2}$	
	73	7.20	72	
	72 $\frac{1}{2}$	7.30	71 $\frac{1}{2}$	
	72 $\frac{1}{2}$	7.40	71 $\frac{1}{2}$	
	72	7.50	71	
	71	8.00	70	
Candles introduced.	70 $\frac{1}{4}$	8.30	69 $\frac{1}{2}$	
	71	9.00	70	
	69	10.00	69	
Deposit all gone.	66	11.00	66	Deposit all gone.

The above results are remarkable. The camphor vapour, as also the small amount of moisture left in the glass, are condensed on the side furthest from the light where the temperature is a little lower than in front; but the most remarkable feature is the scantiness of the camphor deposits, although there was an abundance of light and heat. The deposits were almost nothing, and at length they disappeared. The reason for this is that the front and back temperatures are about equal.

July 27.

Furthest.	Time.	Nearest.	
68 $\frac{1}{2}$ °	8 A.M.	67 $\frac{1}{2}$ °	Three or four lines of scattered deposit nearest the light.
69	9	68	
69	10	68	

Here again is the fact that on this warm and bright morning, when the heat and the light were abundant, the deposits were insignificant. This is easily explained when it is considered that, in consequence of open doors and windows, the temperatures indoors and out are nearly the same. The conditions required for the production of fine deposits are warmth to raise the vapour, and cold on one side to condense it. A room warmed by a fire, and cold air outside, are favourable to the result, so that the camphor-bottles in the window in cold weather are in a better position than in this warm equable temperature.

It may be supposed that the position of the charge in the bottle may have something to do with the motion of the deposit. To settle this point, I prepared four 8-ounce phials of white glass, and put 10 grains of crude camphor into each. In bottle A the charge was in a heap at the back of the bottle, in B in the front, in C at the left-hand side, and in D on the right. The bottles were placed in the window, and fine deposits were formed in all four cases on the side nearest the light; only, in B, C, and D, the deposit was connected with the charge in the same way as smoke may be said to be connected with the fire that produces it. In bottle A there was no such connexion, the deposit being exactly in front, while the charge that supplied it was behind, and no visible connexion between the two.

There is another point which may require notice in these days, when men's minds are so strongly impressed with the actinic action of light. Dr. Draper says that light passed through a solution of bichromate of potash so as not to blacken nitrate of silver, produces an aphelion deposit in a bottle containing camphor. This is a mere effect of the absorption of heat. I arranged a number of flat-sided bottles in pairs, the bottle nearest the light containing a coloured solution, and the bottle in contact with it containing camphor. The narrow sides of the camphor-bottles were made opake, so that the light which passed into them came through the coloured solutions only. These observations were extended over several months, and in every case the camphor produced furthest deposits; and when the bottle was turned round so as to make the furthest deposit a nearest one, it invariably went over to the furthest side. I also enclosed a white glass bottle containing camphor in a wooden box furnished with a

sliding vertical door containing a hole about $2\frac{1}{2}$ inches in diameter, which was accurately closed with a disk of coloured glass. In all cases the deposits were furthest from the light—and naturally so, seeing that a coloured object absorbs the heat more readily than a white one, and keeps the side of the bottle nearest to it of a higher temperature than the other parts.

One more point remains to be noticed. When mercury was exposed to the light in a tall narrow glass, no reliable results were obtained, that is, no deposit was formed that appeared to arise from the condensation of vapour. On two or three occasions metallic tears were seen in the vessel, but it was never clear to me that they did not arise from some shaking or disturbance of the vessel. I could not reproduce even this unsatisfactory result in a narrow vessel, though I carefully tried for it by furnishing the vessel with a cap and stopcock and exhausting it with a syringe. I was also further surprised to find that a barometer-tube of thick glass charged with camphor and exhausted, produced little or no deposit even on the warmest days, and by exposure to direct sunshine. No sooner, however, had I dismissed the action of light from this subject, than the whole matter became clear. A thick glass tube by exposure to the light does not cool unequally, but slowly varies in temperature throughout its mass, so that no deposit either of mercury or of camphor is possible. If, however, the tube be thin, of large diameter and mounted, so that while one part is exposed to radiation the other part is protected, partial cooling is possible, and a deposit is produced. This, too, furnishes an explanation of a fact that had often surprised me. In barometers of large bore there is a deposit of mercury in the Torricellian vacuum on the side nearest the light. I had never seen this in a tube of small bore, though I had frequently looked for it in my own instrument. Some of the barometers of large bore in the International Exhibition have very fine deposits of mercury vapour in the Torricellian vacuum, but in such cases they are mounted so that the tube is more or less exposed. Where the tube is boxed in and protected from radiation there is little or no deposit.

King's College, London,
Long Vacation, 1862.

XLVIII. *Remarks on the Forces of Inorganic Nature.*

By J. R. MAYER*.

THE following pages are designed as an attempt to answer the questions, What are we to understand by "Forces"?

* Translated from the *Annalen der Chemie und Pharmacie*, vol. xlii. p. 233 (May 1842), by G. C. Foster, B.A., Lecturer on Natural Philosophy

and how are different forces related to each other? Whereas the term *matter* implies the possession, by the object to which it is applied, of very definite properties, such as weight and extension; the term *force* conveys for the most part the idea of something unknown, unsearchable, and hypothetical. An attempt to render the notion of force equally exact with that of matter, and so to denote by it only objects of actual investigation, is one which, with the consequences that flow from it, ought not to be unwelcome to those who desire that their views of nature may be clear and unencumbered by hypotheses.

Forces are causes: accordingly, we may in relation to them make full application of the principle—*causa æquat effectum*. If the cause c has the effect e , then $c=e$; if, in its turn, e is the cause of a second effect f , we have $e=f$, and so on: $c=e=f\dots=c$. In a chain of causes and effects, a term or a part of a term can never, as plainly appears from the nature of an equation, become equal to nothing. This first property of all causes we call their *indestructibility*.

If the given cause c has produced an effect e equal to itself, it has in that very act ceased to be: c has become e ; if, after the production of e , c still remained in whole or in part, there must be still further effects corresponding to this remaining cause: the total effect of c would thus be $> e$, which would be contrary to the supposition $c=e$. Accordingly, since c becomes e , and e becomes f , &c., we must regard these various magnitudes as different forms under which one and the same object makes its appearance. This capability of assuming various forms is the second essential property of all causes. Taking both properties together, we may say, causes are (quantitatively) *indestructible* and (qualitatively) *convertible* objects.

Two classes of causes occur in nature, which, so far as experience goes, never pass one into another. The first class consists of such causes as possess the properties of weight and impenetrability; these are kinds of Matter: the other class is made up of causes which are wanting in the properties just mentioned, namely Forces, called also Imponderables, from the negative property that has been indicated. Forces are therefore *indestructible, convertible, imponderable objects*.

in Anderson's University, Glasgow.—Considerable attention having of late been called to the author of this paper, as one of the earliest propounders of the doctrine of the Indestructibility of Force, and especially of the idea of the equivalence of Heat and Work, it will probably interest many readers of the Philosophical Magazine to have placed in their hands his earliest publication on the subject. For some account of Mayer and of his further labours, see Prof. Tyndall's lecture "On Force," Phil. Mag. S. 4. vol. xxiv. pp. 64-66.

We will in the first instance take matter, to afford us an example of causes and effects. Explosive gas, $H + O$, and water, HO , are related to each other as cause and effect, therefore $H + O = HO$. But if $H + O$ becomes HO , heat, *cal.*, makes its appearance as well as water; this heat must likewise have a cause, x , and we have therefore $H + O + x = HO + cal.$ It might, however, be asked whether $H + O$ is really $= HO$, and $x = cal.$, and not perhaps $H + O = cal.$, and $x = HO$, whence the above equation could equally be deduced; and so in many other cases. The phlogistic chemists recognized the equation between *cal.* and x , or Phlogiston as they called it, and in so doing made a great step in advance; but they involved themselves again in a system of mistakes by putting $-x$ in place of O ; thus, for instance, they obtained $H = HO + x$.

Chemistry, whose problem it is to set forth in equations the causal connexion existing between the different kinds of matter, teaches us that matter, as a cause, has matter for its effect; but we are equally justified in saying that to force as cause, corresponds force as effect. Since $c = e$, and $e = c$, it is unnatural to call one term of an equation a force, and the other an effect of force or phenomenon, and to attach different notions to the expressions Force and Phenomenon. In brief, then, if the cause is matter, the effect is matter; if the cause is a force, the effect is also a force.

A cause which brings about the raising of a weight is a force; its effect (*the raised weight*) is, accordingly, equally a force; or, expressing this relation in a more general form, *separation in space of ponderable objects is a force*; since this force causes the fall of bodies, we call it *falling force*. Falling force and fall, or, more generally still, falling force and motion, are forces which are related to each other as cause and effect—forces which are convertible one into the other—two different forms of one and the same object. For example, a weight resting on the ground is not a force: it is neither the cause of motion, nor of the lifting of another weight; it becomes so, however, in proportion as it is raised above the ground: the cause—the distance between a weight and the earth—and the effect—the quantity of motion produced—bear to each other, as we learn from mechanics, a constant relation.

Gravity being regarded as the cause of the falling of bodies, a gravitating force is spoken of, and so the notions of *property* and of *force* are confounded with each other: precisely that which is the essential attribute of every force—the *union* of indestructibility with convertibility—is wanting in every property: between a property and a force, between gravity and motion, it is therefore impossible to establish the equation required for a rightly con-

ceived causal relation. If gravity be called a force, a cause is supposed which produces effects without itself diminishing, and incorrect conceptions of the causal connexion of things are thereby fostered. In order that a body may fall, it is no less necessary that it should be lifted up, than that it should be heavy or possess gravity; the fall of bodies ought not therefore to be ascribed to their gravity alone.

It is the problem of Mechanics to develop the equations which subsist between falling force and motion, motion and falling force, and between different motions: here we will call to mind only one point. The magnitude of the falling force v is directly proportional (the earth's radius being assumed $=\infty$) to the magnitude of the mass m , and the height d to which it is raised; that is, $v=md$. If the height $d=1$, to which the mass m is raised, is transformed into the final velocity $c=1$ of this mass, we have also $v=mc$; but from the known relations existing between d and c , it results that, for other values of d or of c , the measure of the force v is mc^2 ; accordingly $v=md=mc^2$: the law of the conservation of *vis viva* is thus found to be based on the general law of the indestructibility of causes.

In numberless cases we see motion cease without having caused another motion or the lifting of a weight; but a force once in existence cannot be annihilated, it can only change its form; and the question therefore arises, What other forms is force, which we have become acquainted with as falling force and motion, capable of assuming? Experience alone can lead us to a conclusion on this point. In order to experiment with advantage, we must select implements which, besides causing a real cessation of motion, are as little as possible altered by the objects to be examined. If, for example, we rub together two metal plates, we see motion disappear, and heat, on the other hand, make its appearance, and we have now only to ask whether *motion* is the cause of heat. In order to come to a decision on this point, we must discuss the question whether, in the numberless cases in which the expenditure of motion is accompanied by the appearance of heat, the motion has not some other effect than the production of heat, and the heat some other cause than the motion.

An attempt to ascertain the effects of ceasing motion has never yet been seriously made; without, therefore, wishing to exclude *à priori* the hypotheses which it may be possible to set up, we observe only that, as a rule, this effect cannot be supposed to be an alteration in the state of aggregation of the moved (that is, rubbing, &c.) bodies. If we assume that a certain quantity of motion v is expended in the conversion of a rubbing substance m into n , we must then have $m+v=n$, and $n=m+v$; and when n is reconverted into m , v must appear again in some form or

other. By the friction of two metallic plates continued for a very long time, we can gradually cause the cessation of an immense quantity of movement; but would it ever occur to us to look for even the smallest trace of the force which has disappeared in the metallic dust that we could collect, and to try to regain it thence? We repeat, the motion cannot have been annihilated; and contrary, or positive and negative, motions cannot be regarded as $=0$, any more than contrary motions can come out of nothing, or a weight can raise itself.

Without the recognition of a causal connexion between motion and heat, it is just as difficult to explain the production of heat as it is to give any account of the motion that disappears. The heat cannot be derived from the diminution of the volume of the rubbing substances. It is well known that two pieces of ice may be melted by rubbing them together *in vacuo*; but let any one try to convert ice into water by pressure*, however enormous. Water undergoes, as was found by the author, a rise of temperature when violently shaken. The water so heated (from 12° to 13° C.) has a greater bulk after being shaken than it had before; whence now comes this quantity of heat, which by repeated shaking may be called into existence in the same apparatus as often as we please? The vibratory hypothesis of heat is an approach towards the doctrine of heat being the effect of motion, but it does not favour the admission of this causal relation in its full generality; it rather lays the chief stress on uneasy oscillations (*unbehagliche Schwingungen*).

If it be now considered as established that in many cases (*exceptio confirmat regulam*) no other effect of motion can be traced except heat, and that no other cause than motion can be found for the heat that is produced, we prefer the assumption that heat proceeds from motion, to the assumption of a cause without effect and of an effect without a cause,—just as the chemist, instead of allowing oxygen and hydrogen to disappear without further investigation, and water to be produced in some inexplicable manner, establishes a connexion between oxygen and hydrogen on the one hand and water on the other.

The natural connexion existing between falling force, motion, and heat may be conceived of as follows. We know that heat makes its appearance when the separate particles of a body approach nearer to each other: condensation produces heat.

* Since the original publication of this paper, Prof. W. Thomson has shown that pressure has a sensible effect in liquefying ice (*Conf. Phil. Mag. S. 3. vol. xxxvii. p. 123*); but the experiments of Bunsen and of Hopkins have shown that the melting-points of bodies which expand on becoming liquid are raised by pressure, which is all that Mayer's argument requires.—G. C. F.

And what applies to the smallest particles of matter, and the smallest intervals between them, must also apply to large masses and to measureable distances. The falling of a weight is a real diminution of the bulk of the earth, and must therefore without doubt be related to the quantity of heat thereby developed; this quantity of heat must be proportional to the greatness of the weight and its distance from the ground. From this point of view we are very easily led to the equations between falling force, motion, and heat, that have already been discussed.

But just as little as the connexion between falling force and motion authorizes the conclusion that the essence of falling force is motion, can such a conclusion be adopted in the case of heat. We are, on the contrary, rather inclined to infer that, before it can become heat, motion—whether simple, or vibratory as in the case of light and radiant heat, &c.—must cease to exist as motion.

If falling force and motion are equivalent to heat, heat must also naturally be equivalent to motion and falling force. Just as heat appears as an *effect* of the diminution of bulk and of the cessation of motion, so also does heat disappear as a *cause* when its effects are produced in the shape of motion, expansion, or raising of weight.

In water-mills, the continual diminution in bulk which the earth undergoes, owing to the fall of the water, gives rise to motion, which afterwards disappears again, calling forth unceasingly a great quantity of heat; and inversely, the steam-engine serves to decompose heat again into motion or the raising of weights. A locomotive engine with its train may be compared to a distilling apparatus; the heat applied under the boiler passes off as motion, and this is deposited again as heat at the axles of the wheels.

We will close our disquisition, the propositions of which have resulted as necessary consequences from the principle "*causa æquat effectum*," and which are in accordance with all the phenomena of Nature, with a practical deduction. The solution of the equations subsisting between falling force and motion requires that the space fallen through in a given time, *e. g.* the first second, should be experimentally determined; in like manner, the solution of the equations subsisting between falling force and motion on the one hand and heat on the other, requires an answer to the question, How great is the quantity of heat which corresponds to a given quantity of motion or falling force? For instance, we must ascertain how high a given weight requires to be raised above the ground in order that its falling force may be equivalent to the raising of the temperature of an equal weight of water from 0° to 1° C. The attempt to show that

such an equation is the expression of a physical truth may be regarded as the substance of the foregoing remarks.

By applying the principles that have been set forth to the relations subsisting between the temperature and the volume of gases, we find that the sinking of a mercury column by which a gas is compressed is equivalent to the quantity of heat set free by the compression; and hence it follows, the ratio between the capacity for heat of air under constant pressure and its capacity under constant volume being taken as $=1.421$, that the warming of a given weight of water from 0° to 1° C. corresponds to the fall of an equal weight from the height of about 365 metres*. If we compare with this result the working of our best steam-engines, we see how small a part only of the heat applied under the boiler is really transformed into motion or the raising of weights; and this may serve as justification for the attempts at the profitable production of motion by some other method than the expenditure of the chemical difference between carbon and oxygen—more particularly by the transformation into motion of electricity obtained by chemical means.

XLIX. *The Excavation of the Valleys of the Alps.*

By A. C. RAMSAY, F.R.S.†

IN the month of March last I read a memoir to the Geological Society on the Glacial Origin of the Swiss and other Lakes, which has since been published in that Society's Quarterly Journal for August. In that memoir I incidentally alluded (p. 200) to the existence of the chief Alpine valleys *before* the glaciers attained their greatest extension, which valleys were afterwards "*modified* in form by the weight and grinding power of ice in motion."

In a previous memoir, published in 1859, I stated that "it is certain all glaciers must deepen their beds by erosion, and it may be that, when a glacier filled a valley" almost to the brim, "the thickness of the ice was not equal to the present mass added to the superincumbent weight indicated by the signs (striation, &c.) on the slopes above the present surface of the glacier." But though glaciers certainly have a powerful effect in deepening their beds, it has always appeared to me a difficult and perhaps an impossible point to determine to what extent the great Alpine valleys have been eroded by ice—whether, in fact, they have been chiefly scooped out by it, or whether, as I always believed,

* When the corrected specific heat of air is introduced into the calculation this number is increased, and agrees then with the experimental determinations of Mr. Joule.

† Communicated by the Author.

their general contours only have been thoroughly moulded and modified. During a late visit to the Alps I obtained, by help of that distinguished geologist, Mr. Gastaldi of Turin, some important evidence on the subject.

No true geologist is likely to assert that these valleys have been mainly scooped out from end to end by ice, for the reason that, *since* the disappearance of the ice, running water, in the formation of gorges, &c., has comparatively effected so little. Given sufficient time, and, as old Ray long ago inferred, any amount of degradation may be produced by rain and running water. The Alpine streams have doubtless flowed in their present channels for long, and, compared with the depth of the valleys, have done but little. *Long*, however, is a comparative term, and in this instance it may represent a very brief period compared either with the length of the great glacial epoch, or with that which, preceding it, came after the disturbance of those Miocene rocks that form so important a portion of the Alps, and the right understanding of which disturbance is an essential element of the question. For obvious reasons (which it would take long to detail, but which every geologist will appreciate) it is evident that the period that has passed between the disappearance of the great glaciers and the present day is trifling compared with that which elapsed between the close of the Miocene and the commencement of the *glacial epoch* of geologists; and also there is good reason for suspecting that the great glacial epoch itself was longer than that which has passed since the climate of the northern hemisphere ameliorated. It is not therefore to be expected that the later modifications of Alpine valleys produced by existing causes should be commensurate to the old, for *time* is wanting. When we go back to the epochs that followed the disturbance of the Swiss Miocene rocks, we have to deal with periods so long, that (if the style of reasoning of the greatest geologists and naturalists is worth anything) the glacial and existing epochs united are trifling compared with it; for during those periods the mammalian life of the world was renewed again and again, and the invertebrata of the seas, though as species far more enduring, were by slow gradations almost all replaced; while since the commencement of the "glacial epoch" it is more than doubtful if a single species of marine mollusca has disappeared. There was therefore, after the disturbance of the Miocene rocks of the Alps, ample time for enormous degradation by any existing cause.

Let any one who doubts the wasting and erosive power of common atmospheric agents aided by running water, visit Auvergne and the countries formed of solid rocks bordering the Rhine and Moselle. Like the Alps, both of these regions have been above the sea, at least, ever since the close of the Miocene epoch. Both

are unaffected by ice ; and the valleys in them, which are neither few nor small, have all, in the opinion of the best observers, been scooped out by running water during those immense periods of time the relics of which are partly represented by the subdivisions of the Crag and of other strata still more fragmentary, all of which were deposited before the glacial epoch. During all these periods, and many more, of which few traces remain, the Alps were being wasted ; and no geologist acquainted with the evidences of the climate of the times is likely to assert that the great glaciers of the Alps endured during all these changes. The question, therefore, easily arises, to what extent were the Alpine valleys formed during those periods that preceded the "Glacial epoch" *par excellence*, so called from the existence of continental phenomena, both in America and Europe, of which even the old glaciers of the Alps form but a minor part. The evidence is imperfect ; but such as it is, it gives much more than a hint that the large valleys were in their main features approximately as deep as now before they were filled with ice. The belief is as old as the writings of Charpentier ; and others who entertain the notion have only followed in his wake.

At the mouth of the valley of Aosta, the great moraine of Ivrea, which consists of loose material piled on the plain to a height of about 1600 feet, proves that, where the glacier issued from the valley upon the plain, the ice at the sides was at least of that thickness, and probably still thicker in the middle. Further, when lately south of the Alps, it was proved to me by Mr. Gastaldi*, that at the mouths of the great Alpine valleys opening on the plain of the Po, there were ancient alluvial fan-shaped masses of gravel quite analogous to those that by the agency of existing torrents have issued from the gorges on either side (for instance) of the valleys of the Rhone or the Dora, or of those that still issue at their mouths. These were deposited on a plain rather lower than the existing one, above Pliocene marine deposits, at a time when the true mountain-valleys—at all events near their mouths—were just about as deep as they are now ; for the great glaciers that filled the larger valleys issued out upon and overflowed these low-lying river gravels, and deposited their moraines *above* them, only in part scooping them away, apparently because the glaciers did not endure long enough of sufficient size to complete their destruction.

No better proof could be required that in great part the valleys of the Alps were approximately as deep before the glacial epoch as they are at present ; and I believe, with the Italian geologists, that all that the glaciers as a whole effected was only slightly to deepen these valleys and materially to modify their general out-

* See also memoirs by Mortillet and Omboni.

lines, and, further (a theory I am alone responsible for), to deepen them in parts more considerably when, from various causes, the grinding power of the ice was unusually powerful, especially where, as in the lowlands of Switzerland, the Miocene strata are comparatively soft. But for details on this point I must refer to my memoir in the Journal of the Geological Society.

L. *On the Action of Nitric Acid upon Pyrophosphate of Magnesia.* By DUGALD CAMPBELL, *Analytical Chemist to the Brompton Hospital, London*.*

WHEN pyrophosphate of magnesia, obtained by heating to redness the phosphate of magnesia and ammonia, was mixed with nitric acid and heated to about 320° F. for a long time and until the weight was constant, it was found that a large percentage of nitric acid remained with the pyrophosphate; at a very much higher temperature, nitric acid was still retained, and the nitric acid was not entirely driven off until the temperature was raised to a bright red or almost white heat, when it was observed that a considerable quantity of the pyrophosphate of magnesia was also volatilized along with it.

Pyrophosphate of magnesia, dissolved in ordinary nitric acid and exposed in an open capsule to temperatures ranging from 320° F. to 550° F. till the weight became constant for each temperature, was invariably found to have increased very much in weight, although not always to the same extent, as shown below.

Temperature.	Percentage increase of weight.			Difference.
320° F.	22	to	30	8 per cent.
420	19	„	21	2 „
550	13·5	„	14·5	1 „

When the pyrophosphate of magnesia, still retaining nitric acid, but constant in weight at 320° F., was heated sufficiently to drive off all the nitric acid, it was found to have decreased in weight, not to a uniform amount, but varying from 9 to 15 per cent., according to the greater or less rapid application of heat; on heating in the same manner the pyrophosphates of magnesia retaining nitric acid and constant in weight at 420° F. and 550° F., they were found likewise to have decreased much in weight, although not to so great an extent.

It is inferred from these experiments that nitric acid has a stronger affinity for magnesia than pyrophosphoric acid has, and that on adding nitric acid to pyrophosphate of magnesia, nitrate

* Communicated by the Author, being an abstract of a paper read at the Cambridge Meeting of the British Association, October 1862.

of magnesia is formed, pyrophosphoric acid being liberated; and this was proved to be the case by dissolving pyrophosphate of magnesia in nitric acid, evaporating the solution till syrupy, and then placing it under a bell jar over sulphuric acid; after a time, nitrate of magnesia crystallized, and the pyrophosphoric acid could be drained off.

But although nitrate of magnesia is formed and pyrophosphoric acid set free on the addition of nitric acid to the pyrophosphate, it is probable that, when this mixture is evaporated and heated, the products are not always mere mixtures of nitrate of magnesia and pyrophosphoric acid, but that they are sometimes compounds; and the reasons for this opinion are, that these products are but slightly deliquescent, that nitric acid is less readily expelled from them than from nitrate of magnesia, and that on heating these products suddenly, pyrophosphate of magnesia is volatilized, though it is not under ordinary circumstances a volatilizable salt.

In the ordinary mode of estimating phosphoric acid or magnesia, there is great difficulty in obtaining the pyrophosphate of magnesia perfectly white, except by long calcination, and unless a few drops of nitric acid have been added. This plan, which is often recommended and adopted, is likely, however, to lead to considerable errors in the estimation both of phosphoric acid and of magnesia, in consequence of the action above described; and there is no doubt that this mode of operating has been one of the causes which have led to the discrepant results of analyses, and the disputes that have frequently arisen as to the richness of substances in phosphates. It was in the course of some investigations of this kind, some considerable time ago, that the peculiar action of heat upon pyrophosphate of magnesia and nitric acid was first observed.

LI. *Chemical Notices from Foreign Journals.*

By E. ATKINSON, *Ph.D., F.C.S.*

[Continued from p. 311.]

BRIEGLEB and Geuther* have investigated the action of nitrogen on magnesium and some other metals. When magnesium was heated in a current of ammoniacal gas to incipient redness, its volume increased, and externally it became covered with black from the reduction of silicon from the glass tube in which it was heated. The product was dried over sulphuric acid, and, on subsequent fusion with potash, emitted ammoniacal gas. In a subsequent experiment, pellets of magnesium were placed in a porcelain boat and rapidly heated to redness in a current

* Liebig's *Annalen*, August 1862.

of purified nitrogen gas. The weight of the magnesium had increased by about 18 per cent. The substance thus obtained had an almost pure yellow colour: it was generally contaminated by the presence of a little magnesia; for the substance rapidly attracted moisture in the air, forming magnesia and ammonia.

When this substance was heated with hydrochloric acid, it was decomposed with the formation of chloride of ammonium and chloride of magnesium. The analytical results, after allowing for the magnesia formed, agreed with the formula Mg^3N .

Nitride of magnesium forms at ordinary temperatures a greenish mass, which is slowly oxidized to magnesia in dry air, and more rapidly in oxygen. It is rapidly decomposed by water. Acids also decompose it. When heated in carbonic oxide or carbonic acid at a very high temperature, it is decomposed into magnesia with the separation of carbon and liberation of cyanogen. When heated with pentachloride of phosphorus, it forms chloride of magnesium, and a substance which is probably a phosphide of nitrogen, P^3N^5 .

The authors found that iron, when heated in a current of nitrogen nearly to its melting-point, increased in weight about 2.16 per cent., and when afterwards fused with potash, disengaged some ammonia.

Aluminium also appeared to absorb nitrogen at a very high temperature.

Chromium, heated in nitrogen to the strongest red heat, increased in weight to the extent of 15 to 18 per cent. The product obtained had the appearance and all the properties of Ufer's nitride of chromium, Cr^2N , obtained by the action of chloride of chromium and ammonia. The authors did not succeed in obtaining compounds of nitrogen with tungsten or molybdenum.

M. Grandeau* has observed the occurrence of rubidium in the saline residues of beetroot, and in the mother-liquor derived from their treatment for the extraction of chloride of potassium. The specimens examined were from the distillery at Corbehem. M. Lefevre, the proprietor of this establishment, states† that 1 hectare (about $2\frac{1}{2}$ acres) produces 40,000 kilogs. of beetroot, which furnish 128 kilogs. of crude potash. Each kilog. of crude potash gives on the average 1.75 gramme of chloride of rubidium; so that 1 hectare produces 226 grammes, or about $7\frac{1}{2}$ ounces.

To obtain the rubidium-salt, the mother-liquor from the preparation of nitre in the above manufactory is incinerated with wood shavings, to get rid of sulphur and of some nitre; the carbonaceous residue exhausted with water and evaporated to

* *Comptes Rendus*, May 19, 1862.

† *Ibid.* September 8.

one-twelfth, to get rid of the greater part of the salts. Hydrochloric acid is then added to decompose the carbonates, sulphides, and hyposulphites; and to the filtered liquid, nitric acid is added to remove iodine and bromine. The liquid then contains nothing but sulphates and chlorides. It is diluted, heated to boiling, and a saturated boiling solution of chloroplatinate of potassium added. The precipitate formed consists mainly of chloroplatinate of rubidium, which may be purified by treatment with hot water. The chloroplatinate of rubidium is reduced in a current of hydrogen. The residue, treated by water, dissolves out chloride of rubidium.

M. Grandeau has also found rubidium among the salts in Havanna and Kentucky tobacco, in the ashes of coffee and of tea, and in small quantity in the mother-liquors from the preparation of crude tartar.

Than has also* found rubidium in the ashes of the *Quercus pubescens*.

The MM. Becquerel have resumed the researches on the electro-chemical phenomena produced by electrical forces of low intensity. They describe the results obtained with solutions of cobalt, nickel, gold, silver, and platinum.

Cobalt.—This metal is readily obtained pure by the action of a feeble current on a concentrated solution of chloride of cobalt to which an excess of alkali has been added. The metal is deposited in brilliant white coherent tubercles, or in uniform layers, according to the intensity of the current. During its formation some chlorine is disengaged, and some remains in solution as hydrochloric acid; when the solution has attained a certain degree of acidity, the deposit ceases to have the metallic aspect, and takes a blackish appearance. When the solution is again saturated with ammonia, the deposit resumes the metallic lustre. The intensity of the solution must always have a certain relation to the density of the liquid to be decomposed.

The cobalt thus obtained is hard and brittle; reduced at a suitable temperature in hydrogen, it becomes very malleable, and may be worked. With suitable moulds, cylinders, bars, and medals are obtained.

If the solution contains salts of lead or manganese, they are decomposed, and the two metals deposited as peroxide on the positive electrode. Iron remains in solution; no trace is found in the metallic deposit, which is then very pure. The cylinders or bars withdrawn from solution are magnetic, and they have the polarity due to the action of the earth or of the current.

Nickel.—The method is the same; sulphate of nickel is used, to which ammonia must be added. As the decomposition pro-

* Liebig's *Annalen* (Supplement 1), 1862.

ceeds, sulphuric acid becomes free, and the solution must be saturated either by adding oxide of nickel or ammonia. In the first case the solution always remains at the same degree of concentration; in the latter a double sulphate of nickel and ammonia is formed, which is scarcely soluble in water, but soluble in water to which ammonia has been added. At the expiration of some time a white brilliant deposit with a slight yellowish tinge is obtained. It has also magnetic polarity like cobalt.

Gold.—A neutral and very concentrated solution of chloride of gold is rapidly decomposed even by a single element of weak electromotive force, metallic gold being used for the positive electrode. The metal becomes ductile by being annealed. Alkaline solutions need not be added, but the intensity of the current must be proportional to the density of the liquid.

With solution of *silver* the result is the same: the solution must be neutral, the current feeble, and a positive electrode is indispensable.

Platinum is more difficult to obtain as a coherent mass; the negative electrode must be of platinum, round which the metal is often deposited in tubercles.

M. Crova has made a communication* on the formation of cupreous acetylene in copper gas-pipes. Copper gas-pipes which have been long used as conduits for coal-gas frequently give rise to dangerous explosions while they are being cleaned out. M. Crova has made it probable that this may arise from the presence of cupreous acetylene, which detonates at a high temperature or when struck. The author exposed a quantity of reduced metallic copper in a flask containing a mixture in equal volumes of air and acetylene, to which in one case a little ammonia was added. The copper became black, and after the lapse of some time the flask, on being opened under water, showed that about one-half had been absorbed; the absorption in the flask containing ammonia was somewhat greater. The copper was washed and dried; on being treated with hydrochloric acid, a considerable quantity of acetylene was disengaged; when thrown on a heated metallic plate, an explosion was produced, much more violent than with pure cupreous acetylene. As coal-gas always contains acetylene, some traces of air, and even of ammonia, the formation of the acetylene compound can be readily understood.

Pelouze and Cahours† have investigated some of the American petroleum now so extensively used for illuminating and other purposes. The greater part of the oil which they examined boiled regularly at 68°, and formed a colourless and inodorous

* *Comptes Rendus*, September 8, 1862.

† *Ibid.* June 23, 1862.

liquid with an ethereal odour. Its composition was found to be

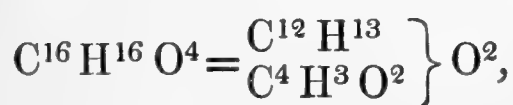


and it is the sixth term in the series of which marsh-gas is the first term. The authors name it *hydride of caproylene*. It dissolves alcohol, ether, fats, and oils. It takes fire when a light is brought near it, and burns with a fuliginous flame. It is unacted upon either by the strongest sulphuric acid, by anhydrous phosphoric acid, or by fuming nitric acid. Chlorine acts upon it, forming a series of compounds in which 1, 2, 3, 4, and 6 atoms of chlorine replace so many atoms of hydrogen respectively.

The first of these bodies, $\text{C}^{12} \text{H}^{13} \text{Cl}$, is the hydrochloric ether of caproylic alcohol. When this body acts upon sulphide of potassium in a closed vessel, chloride of potassium and the *sulphide of caproyle*, $\text{C}^{12} \text{H}^{13} \text{S}$, are obtained. This is a colourless limpid oil with a fœtid odour, boiling at 230° . By the action of hydrosulphate of sulphide of potassium, a compound, $\text{C}^{12} \text{H}^{13} \text{S}$, H S , is obtained corresponding to mercaptan.

When chloride of caproyle is treated with cyanide of potassium, chloride of potassium is formed, and a brown oil, which could not be obtained quite pure, but which appeared to be the cyanide of caproyle. When treated with potash, ammoniacal gas was liberated, and a potash salt formed from which an oily acid was separated on the addition of a strong acid.

The iodide of caproyle, $\text{C}^{12} \text{H}^{13} \text{I}$, is a colourless limpid oil boiling between 172° and 175° , with an ethereal odour like that of iodide of amyle. By the action of this substance on acetate of silver, iodide of silver and acetate of caproyle,



are obtained; by treating this acetate with potash, caproylic alcohol, $\text{C}^{12} \text{H}^{14} \text{O}^2$, is formed. When chloride of caproyle is heated in a closed tube with an alcoholic solution of ammonia, the base *caproylamine*, $\text{C}^{12} \text{H}^{15} \text{N}$, is produced. It is a colourless limpid liquid, with an aromatic ammoniacal odour, and a caustic burning taste. It boils between 124° and 128° . In this action there is also formed a quantity of the base *dicaproylamine*, $\left. \begin{array}{l} \text{C}^{12} \text{H}^{13} \\ \text{C}^{12} \text{H}^{13} \\ \text{H} \end{array} \right\} \text{N}$. It boils at 190° , and has a less ammoniacal odour than the caproylamine.

Bouis and Carlet* have succeeded in transforming œnanthole,

* *Comptes Rendus*, July 21, 1862.

the aldehyde of œnanthylic acid, into its corresponding alcohol. They dissolved a certain quantity of the substance in acetic acid, and placed this mixture along with some zinc in an apparatus which could be heated under a gentle pressure in a water-bath. Under these circumstances the aldehyde, $C^{14}H^{14}O^2$, combined with nascent hydrogen to form the alcohol, which, with acetic acid, formed œnanthylic acetic ether, $\left. \begin{matrix} C^4H^{15} \\ C^4H^3O^2 \end{matrix} \right\} O^2$. By treating this body with potash, acetate of potash was formed, and œnanthylic alcohol, $C^{14}H^{16}O^2$. This body is a colourless liquid, insoluble in water, with an odour like that of caprylic alcohol. It boils at 165° .

Olefiant gas and its homologues have the property of uniting directly with hydriodic, hydrochloric, or hydrobromic acids. Amylene forms with hydriodic acid a hydriodate, $C^5H^{10}HI$, which has the same composition as iodide of amyle prepared from amylic alcohol. Wurtz*, who has recently examined this subject, finds, however, that these substances are not identical. The iodide of amyle boils at 146° , and hydriodate of amylenes at 130° ; and the latter is by no means so stable a substance as the former. Iodide of amyle is acted upon by moist oxide of silver with difficulty, while the hydriodate of amylenes is immediately decomposed. Besides iodide of silver, the principal product of this reaction is an organic hydrate, $C^5H^{12}\Theta$, which Wurtz considers as isomeric with amylic alcohol. It distils between 105° and 108° , and has an odour quite different from that of amylic alcohol. Treated by sulphuric acid it does not give sulphamate of baryta, as does amylic alcohol under these circumstances. Hydriodate of amylenes is acted upon by acetate of silver even at 0° , while iodide of amyle requires the application of heat. The product formed from the hydriodate has the same composition as the acetate of amyle, but is not identical with it; its odour is entirely different. Similar relations are perceived in the case of the hydrobromate of amylenes.

The hydriodic acid is evidently very loosely combined with the amylenes; and it might be supposed that in the hydriodate and in the corresponding hydrate, which might be called *hydrate of amylenes*, the radical amylenes retains its diatomic character. This is the case with the bromide of amylenes; and the hydrobromate has an analogous composition, for HBr is the equivalent of $Br\ Br$. These isomeric relations might be interpreted by the aid of rational formulæ thus,

* *Comptes Rendus*, August 25, 1862.

$(\text{C}^5 \text{H}^{11})' \text{H}$
Hydride of amyle.

$(\text{C}^5 \text{H}^{11})' \text{Br}$
Bromide of amyle.

$(\text{C}^5 \text{H}^{11})' \text{H}\Theta$
Amylic alcohol.

$(\text{C}^5 \text{H}^{10})'' \left\{ \begin{array}{l} \text{Br} \\ \text{Br} \end{array} \right.$
Bromide of amylene.

$(\text{C}^5 \text{H}^{10})'' \left\{ \begin{array}{l} \text{H} \\ \text{Br} \end{array} \right.$
Hydrobromate of amylene.

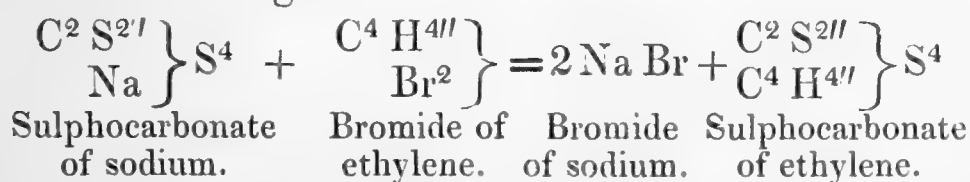
$(\text{C}^5 \text{H}^{10})'' \left\{ \begin{array}{l} \text{H} \\ \text{H}\Theta \end{array} \right.$
Hydrate of amylene.

M. J. M. Crafts has investigated the sulphide of ethylene*, $\text{C}^2 \text{H}^4 \text{S}$. It is best obtained by the action of bromide of ethylene on sulphide of potassium. It forms white crystals, which distil at 200° , and at 112° solidify, forming a crystalline mass. It is acted upon by bromine; and whatever the proportions in which the two substances are employed, the body $\text{C}^2 \text{H}^4 \text{S Br}^2$ is obtained. This is a yellow substance, which is insoluble in ether or in alcohol. It is changed by the action of moist air into white crystals; by the action of water the change is more rapid, and the water contains hydrobromic acid. It is the preceding compound, in which the bromine is replaced by oxygen, $\text{C}^2 \text{H}^4 \text{S}\Theta$.

The author made some experiments on the sulphide of ethylene, an isomer of sulphide of ethylene, obtained originally by Weidenbusch by the action of sulphuretted hydrogen on aldehyde; but the results he obtained showed that the two bodies were quite different in their physical properties.

In a subsequent communication†, the author shows that by the action of nitric acid on sulphide of ethylene the body $\text{C}^2 \text{H}^4 \text{S}\Theta$ is obtained; and by operating with strong nitric acid in closed tubes a binoxide, $\text{C}^2 \text{H}^4 \text{S}\Theta^2$ is obtained, and the liquid contains at the same time a quantity of hyponitrous acid. If the oxidation has not been driven too far, the liquid contains also some of the lower oxide. They are separated by taking advantage of the solubility of the first in water, or, better, by dissolving in strong nitric acid, and adding water, by which the body $\text{C}^2 \text{H}^4 \text{S}\Theta^2$ is precipitated.

Husemann has prepared the sulphocarbonate of ethylene. It is obtained by the action of bromide of ethylene on sulphocarbonate of sodium; chloride of ethylene also yields it, but the action is not so energetic.

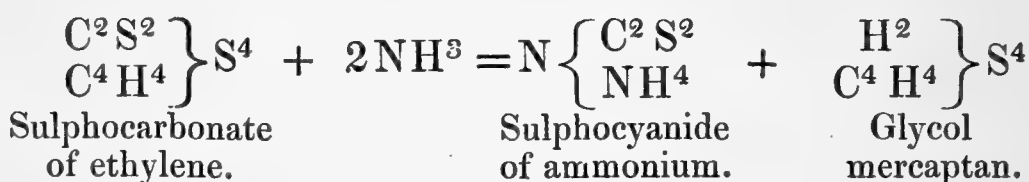


* *Comptes Rendus*, June 23, 1862.

† *Ibid.* August 18, 1862.

With bromide of ethylene the reaction scarcely required ten minutes. At the bottom of the liquid a thick, heavy, oily liquid was found, which was not very soluble in alcohol. This oil could only be partially distilled without decomposition: when heated in a current of hydrogen, a very small quantity of a yellowish crystallized substance was deposited in the neck of the retort. This substance was recrystallized from ether, and obtained in vitreous, well-formed, white crystals, with the further investigation of which the author is engaged. They can be procured in large quantities by the direct action of bromide of ethylene on sulphocarbonate of ethylene. The great mass of the substance remaining in the retort was dissolved in ether and exposed in open vessels to spontaneous evaporation. It was thus obtained in large crystals, which are combinations of a rhombic octahedron, a rhombic prism, and a macrodome. Occasionally they could be obtained 2 to $2\frac{1}{2}$ inches long. The analyses agreed with the formula $C^6 H^4 S^6$.

By the action of ammonia on this substance, sulphocyanide of ammonia is obtained, and the mercaptan of glycol.



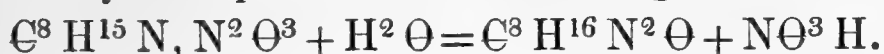
This glycol mercaptan decomposes with evolution of SH into a white pulverulent substance, which appears to be sulphide of ethylene.

The action of fuming nitric acid on the body is very violent, and gives rise to the formation of a new sulphur acid, with the investigation of which the author is still engaged.

Wertheim has published* the results of a very interesting research on coniine. The base was obtained for this purpose from the fresh seeds of *Conium maculatum*, by a method which he described in Liebig's *Annalen*, vol. c. p. 328. When pure coniine was treated with pure dry nitrous acid, the gas was absorbed with considerable disengagement of heat. A certain quantity of the gas could afterwards be expelled by passing dry carbonic acid through the liquid, in which process the colour changed from an emerald to a dark wine-yellow; but by far the greater portion remained absorbed; and the increase in weight corresponded to an absorption of one molecule of $N^2 O^3$ for one molecule of coniine. When this liquid was treated with water, a quantity of a bright yellow oily liquid separated on the surface of the water, which was found to contain nitric acid. The oil was purified by treatment with water and dilute solution of soda, and was then

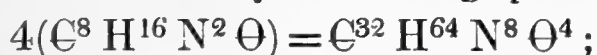
* Liebig's *Annalen*, August 1862.

dried over chloride of calcium. The oily body thus obtained is insoluble in water, but readily so in alcohol and ether: it is quite neutral, and is unacted upon by strong hydrochloric or sulphuric acids, or by strong alkalies. Above 200° C. it decomposes with intumescence. This body is as poisonous as coniine, though slower in its action. It has the formula $C^8 H^{16} N^2 O$, and its formation may be expressed in the following manner:—



New base.

This substance Wertheim names *Azoconydrine*. Some of this substance was placed in a glass bulb, which was then enclosed in a vessel of dry hydrochloric acid gas. On shaking the vessel the bulb was broken, and the contents thus exposed to the action of the gas. The liquid gradually changed almost entirely into a crystalline magma, and on opening the vessel over mercury it was seen that a considerable proportion of the gas had been absorbed. An analysis of the residual gaseous mixture showed that it contained some nitrogen and nitric oxide. The crystals were found on analysis to consist of hydrochlorate of coniine. The decomposition of hydrochloric acid may therefore be expressed as follows, the atom of azoconydrine being quadrupled,



for a determination of its vapour-density showed that this formula corresponded to four volumes,



Azoconydrine.

Hydrochlorate
of coniine.

Another experiment which Wertheim made with azoconydrine is important as tending to throw great light on the nature of coniine. It consisted in heating azoconydrine with phosphoric acid, in an arrangement by which the action could be moderated and at the same time the gases resulting from the action collected. The products of this reaction were an oily liquid, nitrogen gas, and some phosphoric acid. The rectified oily liquid had the specific gravity 0·76076, and the boiling-point 126° C. From an analysis and a determination of its vapour-density, the formula C^8H^{14} was assigned to it. The formation of this substance, which Wertheim names *Conylene*, is expressed as follows:—



Azoconydrine.

Conylene.

Phosphoric acid.

Conylene is probably the biatomic radical of the imide base coniine. When an alcoholic solution of conylene was treated with an alcoholic solution of bromine in such a manner that the action could only take place slowly, a compound of bromine and

conylene was obtained. It is a yellowish oil, of specific gravity 1.57, with an odour like mustard. Its formula is $\text{C}^8 \text{H}^{14} \text{Br}^2$. When this substance, bromide of conylene, is treated with hydrate of potash, it is decomposed into bromide of potassium and a colourless oily substance which is specifically lighter than water, and which is the oxide of conylene, $\text{C}^8 \text{H}^{14} \text{O}$.

The author is still engaged in the investigation of this substance.

LII. *On the Changes in the Apparent Size of the Moon.*

By T. ZENO.

To Professor Tyndall.

SIR,

Guy's Hospital, October 8th, 1862.

MY attention having been called to a letter in the May Number of the Philosophical Magazine, "On the Changes in the Apparent Size of the Moon," where reference is made to my explanation of the phenomenon (Recreative Science, No. 24), I find it necessary to ask your indulgence while I make some allusion to that reference, as well as to the novel and ingenious experiment of your correspondent Mr. Lewis.

First, with regard to the reference to my explanation, it is obvious that, to satisfy the "popular mind" as to the subjective character of the phenomenon, it would have been better to have quoted my second experiment, that of looking at the rising sun till an ocular spectrum was obtained, and then directing the eye to the zenith, where this spectrum immediately diminished, to enlarge again on looking to the opposite horizon. Secondly, with regard to the experiment of Mr. Lewis, it must be noticed that, while it *necessarily* involves the condition of binocular vision, the phenomenon it proposes to illustrate is equally exhibited to monocular vision; the experiment nevertheless, so far as I can judge, very well illustrates that law by which, when two bodies subtend equal angles, both bodies being of the same size, but one assumed to be nearer than the other, the (assumed) nearer appears to be smaller. And though, consequently, the moon in the zenith would appear smaller if *the senses assumed it to be nearer*, the experiment of your correspondent does not hint at the latter condition. Now as the gravamen of the evidence lies here, you will perhaps allow me to state briefly why it is that the luminary (seen with one eye or with both eyes) in the zenith is felt to be nearer than it is when seen in the horizon. I say *felt*, not *reasoned*, inasmuch as this is a sensory perception which no intellection can alter, seeing the setting or rising sun is no less enlarged to the eye of an astronomer (whose reason rejects the illusion) than it is to the eye of the most uninformed rustic.

I propose that the luminary appears nearer at zenith than horizon, because, first (A), the visible sky appears nearer at zenith

than horizon, and, secondly (B), the apparent distance of the luminary accords with that of the sky.

A. Everything visible is seen by rays proceeding from it*. Whatever, then, constitutes the visible sky is thus seen. Now sky would appear black if atmospheric particles did not reflect rays; it would appear white if intervals of black space beyond did not leave points of the retina unexcited; and it appears blue by a law (experimentally deduced by Da Vinci) that retinal excitation (by white light) interspersed with retinal points of rest (derived from the blackness of space) is *the sensory condition blue*. But the (partial) blueness of sky at horizon shows that the blackness of space is not altogether shut out by intervening reflecting particles: whence it follows that the *furthest* reflecting particles of our atmosphere cannot have their light wholly intercepted by the *nearer*; and that the whole atmospheric area contributes, with the blackness of space beyond, to constitute the visible sky. The whole atmosphere, then, gives the *real* locality of that we call visible sky—seen by rays proceeding from it (its varying shades, from blue to nearly black, in various latitudes, showing that colour is its visible accident and not its visible essence). Now, whether we draw a line from the spectator to the limit of the atmosphere or to any assumed point within it, the line in direction of horizon will be longer than in that of zenith; and in whatever form any part, or the whole, of this transpicuous extent of atmosphere is manifested to vision by the aforesaid rays, such manifestation in direction of horizon is more or less *obscured*, in consequence of the rays depicting it coming from the greater distance and under a smaller angle; in other words, every visible quality and attribute of the sky will appear more distinct at the nearer point (zenith) than at the further point (horizon)†.

But if this necessarily curtailed demonstration should not be fully conclusive, the *facts*, reasoned to, are obvious; the sky is more definite and more blue in appearance at the zenith: moreover we naturally express our *sensation* by naming it the “near sky,” and that at the horizon the “far sky.” The synthetic

* For, though perfect blackness, with its retinal equivalent non-excitement, is often an instrument of vision, an object from which no rays proceed is, *per se*, invisible.

† Another source of obscuration, owing to the greater density of the lower strata of atmosphere (a consideration that has misled some writers on this subject), is intentionally omitted, as also (further on) obscuration of the luminary itself, from the same cause. For, first, the senses (which are much more wakeful observers than the reason) are conversant with the fact of the bases of high buildings, trees, vertical cliffs, &c. being more obscured than their summits, *in which case* the sense knows that greater obscuration does not mean greater distance; secondly, my second experiment, already stated, proves that our phenomenon is, or may be, independent of this obscuration.

argument, finally, seems irrefragable: engraver and painter succeed in presenting, the one the form, and the other the form and colour of the sky as we really perceive them by the sense, their success depending solely on an *observed law of perspective*, which mathematically demonstrates the sky they depict at the horizon to be further considerably than that they represent at the zenith. Now the senses, in recognizing the truth of this representation of the sense-perceived form of the sky, assert unequivocally what the sense-perception is.

It remains to show, B, that the apparent distance of the luminary coincides with that of the sky. It may be argued, since there is no sensory evidence of the *actual distance* of the luminary, 1st, that the sense can only refer it to the ground which surrounds it; 2nd, that, by the universal law determining visual distance with one or both eyes, no object is felt to be beyond or in advance of another surrounding it, unless, the size, parts, texture, or colour of both being known or assumed, the sight detects that one is more obscured than the other, in which case the less obscured is inferred to be less distant, and *vice versa*; 3rd, a strong light, placed at any distance behind a hole in a semi-illuminated screen, appears to fill the aperture and be there situate; or a sunbeam piercing an interstice appears to emanate from the object in which the interstice exists.

If, then, the positions A and B are established, it follows that the luminary in the zenith appears nearer than at horizon; and, subtending (practically) the same angle in both cases, it is hardly necessary to add (what my paper referred to sets forth) that when the luminary is supposed to be nearer it is necessarily perceived to be smaller.

Trusting you will excuse the unforeseen length of this explanation of my views, perhaps somewhat misunderstood by your correspondent,

Believe me to be, Sir,

Yours very respectfully,

T. ZENO.

LIII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 322.]

November 21, 1861.—Sir Benjamin Collins Brodie, Bart., President, in the Chair.

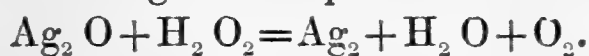
THE following communications were read :—

“Note on the Oxidation and Disoxidation effected by the Peroxide of Hydrogen.” By B. C. Brodie, F.R.S., Professor of Chemistry in the University of Oxford.

In a former communication* I laid before the Society a detailed

* Phil. Trans. ii. 1850, 759.

investigation as to the remarkable disoxidation of certain metallic oxides (of which the decomposition of the oxide of silver may be regarded as typical) by the peroxide of hydrogen. I suggested that this decomposition was of a simple and normal chemical character; that the element oxygen was formed according to a molecular law identical with that according to which compound substances are formed; and that the mutual decomposition of the two oxides was determined by the synthesis of the particles of oxygen in opposite polar conditions, according to the equation



After an interval of nearly ten years, this explanation, together with various facts bearing upon the theory of this action, and which were given for the first time in the paper alluded to, have been reproduced as original discoveries by Schönbein, Professor of Chemistry at Basle*.

The reaction does not, however, present itself under this simple form. The amount of oxygen lost by the oxide of silver is a variable quantity, comprised between the extreme limits of the catalytic action in which the reduction is zero, and the normal chemical action as expressed in the above equation. The reason for this variation is to be sought in the disturbing influence of the metal formed during the decomposition.

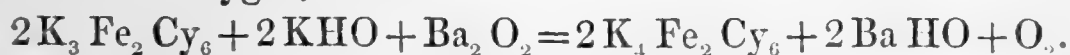
It is my intention shortly to communicate to the Society a prosecution of the above inquiry, in which I investigate the decomposition by the peroxide of hydrogen of certain oxygenated substances contained in solution, where the perturbing causes which affect the decomposition of the solid oxides and conceal the simplicity of the reaction, do not exist.

The methods by which the following results have been obtained will hereafter be given in detail. I confine myself to a brief *résumé* of the conclusions.

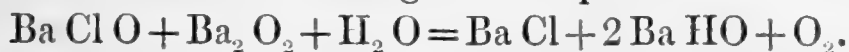
(1)† When an acid solution of permanganic acid is decomposed by peroxide of hydrogen, the decomposition is in an invariable atomic proportion, according to the equation



(2) An alkaline solution of ferricyanide of potassium is reduced by the peroxide of barium to ferrocyanide, with the evolution of two atoms of oxygen, thus



(3) An alkaline solution of hypochlorite of barium is reduced by the peroxide of barium, according to the equation

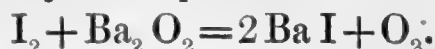


A similar change takes place, as might be anticipated, with an

* Phil. Mag. [4] xvi. 178.

† This reaction has, I find, been published by Aschoff (see Repertoire de Chimie Pure, August 1861, page 296). It was last year made the subject of a communication by me to the British Association, "On the Quantitative Estimation of the Peroxide of Hydrogen."

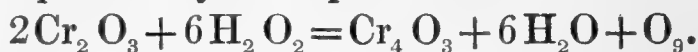
aqueous solution of chlorine in the acid solution of peroxide of barium, but more slowly. I have elsewhere shown that the action of iodine is expressed by the equation



(4) The decomposition of chromic acid is of special interest. In this case there are two distinct reactions, the first of which takes place when chromic acid is in excess, according to the equation



the second, which occurs when a large excess of peroxide of hydrogen is present, and in which the peroxide of hydrogen loses exactly double the amount of oxygen lost by the chromic acid, so that the final result is expressed by the equation



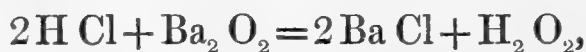
In the interval between these reactions the ratio of the loss of oxygen from the peroxide to the loss from the chromic acid varies between the limits 1 and 2 according to a definite law, being proportional to the ratio of the masses of the substances employed. I am able to show, by direct experiment, that the reaction between the extreme limits is the sum of two distinct and simple atomic decompositions.

The alkaline peroxides which produce these peculiar effects of reduction, under other conditions act as powerful oxidizing agents; thus an acid solution of ferrocyanide of potassium is oxidized by peroxide of hydrogen to ferricyanide. Hydrated protoxide of manganese is oxidized by peroxide of barium to the condition of peroxide of manganese. A concentrated solution of hydrochloric acid evolves chlorine from peroxide of barium.

This last reaction I have made the subject of a special investigation, with the view of determining the conditions under which the two reactions,



and



respectively take place; and I find that when a solution of hydrochloric acid is boiled with peroxide of barium, if the solution be concentrated, chlorine, if the solution be dilute, oxygen, exclusively is evolved; and that for each intermediate degree of dilution chlorine and oxygen together are evolved in a constant ratio.

It is thus seen that those differences in the behaviour of the different classes of peroxides, from which an imaginary distinction has been drawn between the oxygen respectively contained in them as positive or negative, are not fundamental and characteristic differences. The oxygen in the peroxide of barium is the same as that in the peroxide of manganese, according to the only test of identity which we can apply, for we can produce with it the same effects. Nor are the peculiarities in the reactions of the oxygen of the alkaline peroxides of such a nature as to need any special hypothesis to account for them; for in no case are the combining properties of the particles of matter, like their atomic weights, constant for

each chemical substance, but they are variable properties, depending on the physical conditions in which the particles are placed, and the chemical substances with which they are associated.

“On the Aquiferous and Oviducal System in the Lamellibranchiate Mollusks.” By George Rolleston, M.D., F.L.S.; and C. Robertson, Esq.

“Notes of Researches on the Poly-Ammonias.—No. XVI. Triatomic Ammonias.—No. XVII. Mixed Triammonias containing Monatomic and Diatomic Radicals.—No. XVIII. Tetrammonium-Compounds.” By A. W. Hofmann, LL.D., F.R.S.

“On the Contact of Curves; and on the Calculus of Functions.” By William Spottiswoode, Esq., M.A., F.R.S.

“On the Action of Hydriodic Acid upon Mannite.” By J. A. Wanklyn, Esq., and Dr. Erlenmeyer.

“The Lignites and Clays of Bovey Tracey, Devonshire.” By William Pengelly, Esq., F.G.S.

“The Fossil Flora of Bovey Tracey.” By Dr. Oswald Heer.

December 5, 1861.—Major-General Sabine, R.A., President, in the Chair.

The following communications were read:—

“On Crystallization and Liquefaction, as influenced by Stresses tending to change of form in Crystals.” By Professor James Thomson, Queen’s College, Belfast.

In a paper submitted to the Royal Society, and printed in the *Phil. Mag.* for May 1862, I directed attention in a note (page 409), to the question of how the *surface* of a bar of ice in ice-cold water, as distinguished from the interior of the bar, may, by the application of tension to the bar, be influenced in respect to tendency either to melt away, or to solidify to itself additional ice from the water; but did not then venture to offer a positive answer. I pointed out as a matter deserving of special attention, and as affording scope for much additional theoretical and experimental investigation, the distinction between the application to ice in ice-cold water, of stresses tending to change its form, the stresses not being participated in by the water; and the application directly to the water, and through that to the ice, of cubical or hydrostatic pressures or tensions, these being participated in by the water and the ice alike; and I pointed out that the theory and quantitative calculation which I had originally given* of the effect of pressure in lowering the freezing-point of water, or of diminution of pressure in raising it, applied solely to effects of pressure communicated to the ice *through the water*, and therefore equal in all directions, and equally occurring in the ice and the water; but that when changes of pressure in one or more directions are applied to the ice as distinguished from the water, the theory does not apply in any precise way to determine the conditions of the

* Transactions Roy. Soc. Edin. vol. xvi. part 5, 1849; and Cambridge and Dublin Math. Journ. Nov. 1850.

melting of the ice, or of its growth by the freezing of the adjacent water to its surface ; and I expressed the hope that I might subsequently communicate to the Society some further developments of the subject.

On following up various considerations which had then occurred to me, I soon formed positively the opinion that *any stresses whatever, tending to change the form of a piece of ice in ice-cold water* (whether these stresses be of the nature of pressures or tensions, that is pushes or pulls, and whether they be in one direction alone, or in more directions than one), *must impart to the ice a tendency to melt away, and to give out its cold, which will tend to generate, from the surrounding water, an equivalent quantity of ice free from the applied stresses.* I came also to the more general inference that *stresses tending to change the form of any crystals in the saturated solutions from which they have been crystallized must give them a tendency to dissolve away, and to generate, in substitution for themselves, other crystals free from the applied stresses or any equivalent stresses.* In the month of May last, I tested this inference by applying stresses to crystals of common salt in water saturated with salt dissolved from the crystals themselves ; and found the crystals to give way gradually, with a plastic yielding, like the yielding of wet snow, but very much slower. The crystals, with the brine in which they were immersed, were, in the first set of experiments, placed in a glass tube, like a test-tube, and a glass piston, or rammer, fitting the tube loosely, so as not to be water-tight, was placed on the top of the salt which lay like fine sand in the bottom, and the piston was loaded with weights. The piston went on descending from day to day through spaces, which, though small, and though diminishing as the crystals became more compacted against one another, were still distinctly visible. When the rate of descent became very slow, I added more weights, and found that the rate of descent increased, as was to be expected. I afterwards procured a strong brass cylinder with a loosely fitted, not water-tight piston, or rammer, and in this I subjected crystals of common salt in their saturated brine to very heavy stresses, and thus compressed them rapidly and easily into a hard mass like rock-salt. The top surface presented a perfect impression of the tool marks on the bottom of the piston, such as might have been made in wax. The expulsion of the minute quantities of brine remaining in pores in the salt when it has become very closely compacted, appears to be a slow and difficult process ; as, after the pressure had been continued for about a fortnight, I still found a slight oozing of brine from a pore which happened to exist in the side of the cylinder.

Experiments by the application of tensile stresses, or of any other stresses than those mixed and chiefly compressive ones which arise when the crystals are pressed in a close vessel by a rammer, would probably not be very easily carried out ; and I have not as yet tried any except those by pressure. I feel quite convinced, however, that melting, or dissolving, must result from all kinds of stresses tending to change of form. I think the following statement may be assumed

as a general physico-mechanical principle or axiom, and I think it involves the truth of the opinion just expressed :—

If any substance, or any system of substances, be in a condition in which it is free to change its state (whether of molecular arrangement, or of mechanical relative position and connexion of its parts, or of rest or motion), and if mechanical work be applied to it (or put into it) as potential energy, in such a way as that the occurrence of the change of state will make it lose (or enable it to lose) (or be accompanied by its losing) that mechanical work from the condition of potential energy, without receiving other potential energy as an equivalent ; *then the substance or system will pass into the changed state*. The consideration of a few cases, in some of which there is not freedom for the substance or system to change its state, and in others of which there is freedom, will render the meaning of this more clear.

Gunpowder may be cited as an example of a substance in a condition *not free to change its state*, although when it is made to explode by a spark, it passes to an altered condition, and, in doing so, even gives out a great amount of mechanical work. That is to say, that *on the whole* it is more than free to change to the exploded state, or it tends so to change, but there is some kind of obstacle at ordinary temperatures, to the change, which either vanishes at a high temperature, or requires the application of mechanical work to begin the overcoming of it. When the change is once begun, the requisite help is given to the succeeding parts by those which have gone off first.

Again, water confined in a high reservoir is not free to go to a lower one ; although a siphon, primarily filled with water, may help the parts successively over the obstacle by lending to each the requisite mechanical work in advance, which it afterwards pays to the parts which are to follow, besides that it gives out in its fall a great additional amount of power or energy applicable otherwise. Two reservoirs of water, on the same level, and having an opening between them under the water surface, would represent the case of *perfect freedom for change of state* ; and two on a level with one another, but separated by a partition, would represent the case in which no mechanical work would finally be either given out or absorbed by the change, but in which there is not perfect freedom to change, until a siphon or other means of help is applied.

A bell hung from an axle and then turned up, and left resting against a stop a little beyond its position of unstable equilibrium, is not free to go down, but a slight pull will bring it over this position and make it free to swing, which the work stored as potential energy in the raising of it from its low or hanging position, will cause it to do ; its fall till it comes to the bottom being essentially accompanied by the loss of that potential energy, as such, though not as actual energy, out of the system of which it and the earth are the two parts, and in which change of their distance asunder constitutes change of their potential energy.

If in an atmosphere of steam resting on water at its boiling temperature for the pressure of the steam ; as, for instance, in the inside

of a boiler partly filled with water, and partly with steam, an inverted cup, or bell-shaped vessel, be suspended, and if it then, being full of steam, be forced down under the water, mechanical work will be imparted as potential energy to the system of which the steam and water in the boiler form one part, and the earth is the other part ; though, for brevity of expression, the work may be spoken of as applied to the steam and water. In this case there is perfect freedom for the steam forced under the water to condense and cause by communication of its latent heat the generation of an equal quantity of steam at the surface of the water under which the bell was sunk. The occurrence of this change of state will enable the system to lose the potential energy which had been imparted to it by the submersion of the steam, or will release that energy which had been stored, and the system *will pass into the changed state* ; that is to say, a certain part of the steam will change to water, and, instead of that, a different part of the water will be changed to steam ; and this change will be accompanied by a transmission of heat from the part condensing to the part evaporating. This is all in accordance with the axiom ; and we know otherwise that it must take place, as the steam being pressed when submerged must condense and give its latent heat to the water, and that heat must generate an equal quantity of steam at the surface of the water, where the pressure is less. Thus the truth of the axiom is confirmed.

If a quantity of ice and water be enclosed in a cylinder with a water-tight piston, and if this be put into a completely closed vessel filled with other ice and water, and if the piston then be pulled with any given force and fixed in its new position (which might be done in many ways, as for instance, by the use of an axle passing air-tight through the side of the outer vessel), mechanical work will be introduced as potential energy into the system consisting of all the things enclosed in the outer vessel. But there is perfect freedom for the water enclosed in the cylinder to proceed to freeze, obtaining the requisite cold from the ice in the water confined around the cylinder and within the outer vessel. The occurrence of this change would be accompanied by the system's losing or giving up the potential energy which had been stored in it. According to the axiom, then, the change ought to occur. But we know otherwise that it must occur ; because the diminution of hydrostatic pressure in the cylinder raises the freezing-point of the enclosed water, and makes it freeze by the cold of the surrounding mixture of ice and water, which, besides, by being itself subjected to increased pressure, tends to give out cold by the lowering of its freezing-point. Thus the truth of the axiom is again confirmed.

Lastly, if a bar of ice in ice-cold water be subjected to any stress (a pull for instance) tending to change its form, it will receive mechanical work from the force, or forces, applied, and that work will be stored as potential energy in the elasticity of the ice. Now, if there be another piece of ice in juxtaposition with this piece, seeing that, at the beginning, both these pieces were free from externally applied forces, and were both in the state in which either was perfectly

free to melt and cause an equal quantity of water to freeze to the other*, it will follow according to the axiom, now supposed to be established, that the application of the stress *will cause this action to occur*.

The case of crystals in their solutions might be stated almost in the same words as the case of ice in ice-cold water: but it is to be observed that, in their case, the necessity for the translation of one chemical substance through another (the salt through the dissolving liquid), and not of heat or cold alone, causes a great slowness of the process, as compared with that of the yielding of the ice, in ice-cold water, to applied stresses.

At an early stage of the considerations which led to the opinions on the influence of stresses on crystallization and liquefaction described in the present paper, the question arose to me:—Is a spiculum or single crystal of ice, which has solidified itself in the interior of water, and is therefore not colder than the water, plastic? Or would it, when in the water, and attached by one end, as for instance to a crust of ice lining the containing vessel, gradually bend upwards by its own bouyancy in the heavier water? My idea is that it is not plastic. I cannot conceive of the growth of a crystal proceeding with one continuous or uninterrupted structural arrangement, if during its growth the part already formed undergoes permanent change of form, such as would be due to any plastic or ductile yielding. I think we must suppose the molecules in the interior of one crystal to be so locked into one another, by the forces of crystalline cohesion, that any one of them, or set of them, would experience a difficulty in making a beginning of the change of state from solid to liquid. I have not succeeded even in forming any clear conception of continuous crystalline structure admitting of what may be called ductile or malleable bending (that is, bending beyond limits of elasticity such as occurs in lead, copper, tin, and many other metals), and still remaining of the nature of one continuous crystal. What in soft or malleable crystals of copper or other metals, deposited in the electrotpe process, may be the nature of the change of molecular arrangement induced

* The supposition here assumed, however, of there being perfect freedom for either of two pieces of ice, which are immersed in the same water, and are alike free from stresses, to melt, and, by giving out its cold, to cause an equal quantity of water to freeze to the other, will probably not meet with assent at present from all, as it appears to be a prevailing opinion that water and ice in contact are *not* in a state of perfect indifference as to retaining or interchanging their conditions. It is supposed that ice has a property of tending to solidify water in contact with it, and the more so if there be ice on both sides of the water than if on only one side. Again, it is supposed that ice is essentially colder than water in contact with it, and that the water must continually be giving off heat to the ice. Both these opinions are inconsistent with the supposition here assumed. I conceive, however, that that supposition is amply confirmed by the fact that it was involved essentially throughout the reasoning, by which I was led to conclude that the freezing-point is lowered by increase of pressure, and to calculate the amount of the lowering. That reasoning led to true results, and I believe it could not have done so unless the supposition were true, that when water and ice are present together their freedom to change their state on the slightest addition or abstraction of heat, or on the slightest application of mechanical work tending to the change, is perfect.

by bending them, I cannot say ; but I suppose that, in their yielding, their crystalline structure is materially altered, and rendered discontinuous where, before, it was continuous.

In a mass of plastic ice, I incline to think that the internal melting, to which I attribute the plasticity, must occur at the surfaces of junction of separate crystals or fragments of crystals ; though probably pores formed by melting, by pressures, or by stresses, may penetrate crystals by entering them from their moistened surfaces or their junctions with other crystals. It now becomes clear, I think, that the influence of stresses affecting the ice, and tending to make it melt without there being necessarily any consequent pressure applied to the water in contact with the ice, must come to be taken into account in any theory of the plasticity of ice approaching to completeness. This view does not, however, I think, supersede the theory of the plasticity of ice sketched out by myself in former papers, but rather constitutes an amendment, and further development of it. Any complete theory of the plasticity of ice, and of the nature of glacier motion, must comprise the conditions as to fluid pressure and structural arrangement of the water and air included in the ice, and must so explain the lamination of the glacier, seen as blue and white veins. My brother, Professor William Thomson, in papers in the *Phil. Mag.* for October and December 1858, endeavoured to follow up my previously published views on the plasticity of ice with an explanation of the laminated structure, based on the same principles. The explanation he then offered, I think, cannot fail to assist in suggesting the direction in which the true solution is ultimately to be sought for ; yet I feel confident that no full and true solution has as yet been found*.

In the foregoing part of the present paper, I have shown reason why stresses applied to crystals when in contact with the liquid from which they have been produced, should be expected to cause them to melt or dissolve away. The following line of reasoning to show that stresses applied to a crystal will cause a resistance to the deposition of additions to it from the liquid, or, in other words, a resistance to its growth, will, I think, prove to be correct. When a crystal grows, the additions, it seems to me, must lay themselves down in a state of molecular fitting, or regular interlocking with the parts on which they apply themselves ; or, in other words, they must lay themselves down so as to form one continuous crystalline structure with the parts already crystallized. It thus seems to me that, if a crystal grows when under a stress, the new crystalline matter must deposit itself in the same state of stress as the part is in on which it lays itself. If, then, we consider a spiculum of ice growing in water, and if we apply any stress, a pull for instance, to it while it is thin, and

* I have my brother's authority for stating that, although he believes the physical principles suggested in his papers here referred to to be capable of being developed into a true explanation of the phenomena, yet he considers further investigation necessary, and does not feel confident as to the correctness of that part of the explanation he offered, in which the mutual action of two vesicles in a line oblique to that of maximum pressure is considered.

then fix it in its distended state, and if then by the transference* to the water beside it of cold taken from any other ice at the freezing-point we cause it to grow, which it may do if there be no other crystal of ice beside it more free than it to receive accessions, then the additional matter will, I think, lay itself down in the same state of tensile stress as the original spiculum was put into by the applied pull. The contractile force of the crystal will thus be increased in proportion to the increase of its cross sectional area. If it now be allowed to contract and relax itself, it will give out, in doing so, more mechanical work than was applied to the original spiculum during distention. Hence there would be a gain of mechanical work without any corresponding expenditure; or we could theoretically have a means of perpetually obtaining mechanical work out of nothing, unless it were the case that greater cold is required to freeze water into ice on the stressed crystal than on a crystal free from stress. Hence we must suppose that a greater degree of cold will be required to cause the stressed crystal to grow. The reasoning just given has been for brevity stated somewhat in outline; but I trust the full meaning can readily be made out, and that what has been said may suffice.

I wish now to suggest as an important subject for investigation, The Effect of Change of Pressure (hydraulic pressure) in changing the Crystallizing Temperatures of Saline or other Solutions of given Strengths,—as I feel sure that such effect must exist, but am not aware that it has been hitherto discussed or experimented on, and as it is intimately connected with the matters under consideration in the present paper and with subjects discussed in previous papers, which I have submitted to the Royal Society, on Ice.

“Determination of the Magnetic Declination Dip, and Force, at the Fiji Islands, in 1860 and 1861.” By Colonel William James Smythe, of the Royal Artillery.

“On the Calculus of Functions.” By W. H. L. Russell, Esq., A.B.

“On Tschirnhausen’s Transformation.” By Arthur Cayley, Esq., F.R.S.

December 12.—Major-General Sabine, R.A., President, in the Chair.

The following communications were read:—

“On a Series for calculating the Ratio of the Circumference of a Circle to its Diameter.” By Amos Clarkson, Esq.

“On the Production of Vibrations and Sounds by Electrolysis.” By George Gore, Esq.

In this communication, which is a continuation in subject (but different in title) of a previous investigation “On the Movements of Liquid Metals and Electrolytes in the Voltaic Circuit,” the author has

* A theoretic air-engine for making such transferences of heat or cold was used in the reasoning by which I determined theoretically the lowering of the freezing-point by pressure; and the same is admissible here.

described the most convenient and effective method of obtaining vibrations and sounds by electrolysis.

The paper contains a full account of the influence of various circumstances upon the vibrations and sounds: viz., of the electrodes,—the electrolyte,—mechanical circumstances and temperature,—the electric current,—size and number of voltaic elements,—quantity of the current,—coils of wire in the circuit,—induction coils and iron cores,—electrolytes in the circuit,—and of magnetism: also the influence of the vibrating medium itself upon the electric current.

The best liquid for producing the vibrations and sounds consists of 10 grains of cyanide of mercury and 100 grains of hydrate of potash dissolved in $2\frac{3}{4}$ ounces of aqueous hydrocyanic acid containing 5 per cent. of anhydrous acid.

The vibrations and sounds occur only at the surface of the negative mercury electrode, as already described (Phil. Mag. S. 4. vol. xxii. p. 555); and out of a large number of liquids examined, the only ones in which *phonetic* vibrations occurred were solutions of alkaline cyanides containing dissolved mercury; and these only give the sounds with electrodes (or at least a cathode) of mercury, not with solid metals nor with fused alloys.

The vibrations and sounds vary considerably according to the size and number of the voltaic elements; with a few elements of large surface, the vibrations were small and the sounds high, and with many elements of small surface they were much larger and the tone lower. The most suitable number of elements to produce them is either two of Grove's or five of Smee's.

The interposition of a coil of stout copper wire in the circuit made the vibrations wider and the sound more base; and if an iron core was suddenly thrust into the axis of the coil, they became still wider and the sounds still more base, and remained so as long as the iron continued there; but if a secondary coil containing a great length of fine copper wire surrounded the primary coil (with or without an iron core), and the ends of the secondary wire were suddenly united, the vibrations instantly became narrower and the sounds more high, and remained so as long as the secondary circuit was closed.

The vibrations of the mercury and electrolyte make the electric current which produces them sensibly *intermittent*, similar to the influence of a vibrating coil-hammer; and they may be used to some extent in a similar way to that instrument to produce shocks, &c. by means of a secondary coil. A strong electro-magnet placed in various positions near the locality of the vibrations had no perceptible influence upon them.

The author considers the vibrations to be of *electro-chemical* origin, and to result from an attraction between the mercury of the negative electrode and the mercury of the electrolyte. He supposes that to produce the vibrations, either the voltaic force itself must be of an intermittent nature, or the resistance opposed to that force by the liquids employed must be intermittent, and intends to make the experimental investigation of this question, with other allied matters, the subject of a future communication.

“On Perchloric Acid and its Hydrates.” By Henry Enfield Roscoe, B.A., Ph.D., Professor of Chemistry in Owens College, Manchester.

January 9, 1862.—Major-General Sabine, President, in the Chair.

The following communications were read :—

“Preliminary Note on the Nature and Qualities of Voltaic Currents.” By George Gore, Esq.

1. In a recent paper “On the production of Vibrations and Sounds by Electrolysis,” I have shown that if a voltaic current of suitable quantity from two Grove’s or five Smee’s elements is passed by a mercury anode through a solution composed of 10 grains of cyanide of mercury and 100 grains of hydrate of potash, dissolved in $2\frac{3}{4}$ ounces of hydrocyanic acid containing 5 per cent. of anhydrous acid, into an annular cathode of mercury about 2 or 3 inches diameter and $\frac{1}{8}$ th of an inch wide, *visible* and symmetrical *vibrations* of the negative mercury, accompanied by definite *sounds*, are produced; and the current becomes *intermittent* as long as the vibrations continue, similar to a current made intermittent by means of an ordinary break-hammer.

2. If two voltaic currents of suitable and *equal quantity* (measured by a voltmeter in the circuit), the one being generated by about eight Smee’s elements of large immersed surface, and the other generated by twenty Smee’s elements of small surface, are passed separately through the arrangement just described, vibrations and sounds are in each case produced; but with the current from the few elements of large surface the amplitude of the vibrations is small and the sound high, whilst with the current from the many elements of small surface the amplitude of the vibrations is large and the sound base. These differences in the vibrations and sounds are still more conspicuous if a galvanometer of small resistance (*i. e.* with a short and thick wire) is substituted for the voltmeter, and about four Smee’s elements employed instead of the eight. In each of these experiments the voltmeter (or galvanometer) is in the circuit with the cyanide solution; the quantities of the two currents are made equal by suitably adjusting the relative depths of immersion of the plates of the two batteries; and each experiment (with the voltmeter) occupies 3 minutes. The size of the mercury electrodes has also been previously adjusted to the power of the current, so as to give continuous definite vibrations and sounds.

3. Further:—If a current from two Grove’s or five Smee’s elements of large surface is passed through a primary coil of about 250 feet of thick copper wire, through the cyanide solution and small-resistance galvanometer, the vibrations are moderate in size and the pitch of the sound is moderately high; but if the axis of the coil contains a massive bundle of soft iron wires, the vibrations are much larger and the pitch of the sound is much more base; and if the primary coil is surrounded by a secondary coil containing about 4000 feet of fine copper wire, the ends of which are closely united

together, and the iron core is absent, the vibrations are very much smaller and the sound is much higher. In each case the *quantity* of the current, however, remains the same. If a voltameter is used instead of the galvanometer, a greater number of elements (about eight Smee's) must be employed, and the difference in the effects is then less striking. If a battery of much greater intensity, say twenty Smee's elements, is employed, no difference in the vibrations or sounds is produced by the introduction of the soft iron core, nor by closing the secondary coil.

Do not voltaic currents therefore of equal quantities from different sources, or under different external conditions, like heat and light from different sources, possess different qualities?*

4. From these results (as well as from additional ones that I have obtained) it appears to me that voltaic electricity, like heat or light, may be viewed as consisting of *vibrations* or *successive impulses*, which under ordinary circumstances occupy so minute a period of time as to be inappreciable, but when acting under suitable conditions upon suitable substances, such as the metal and liquid referred to (1), the vibrations of the current are taken up by the substances, and the oscillations of the substances thereby produced are gradually increased by the synchronous impulses of the current until they become visible and attain their maximum (see paper "On the production of Vibrations and Sounds by Electrolysis," paragraph 11), like visible oscillations of a pendulum produced by minute synchronous mechanical impulses. This I beg leave to state as an hypothesis for the purpose of making the subject more clear and aiding future inquiry.

Note by Professor STOKES, Sec. R.S.

[The results mentioned in this paper are well worthy of attentive consideration, in relation to that curious and still mysterious phenomenon which the author is investigating with so much care. As regards, however, the conjecture thrown out by the author,—while the importance of such a conclusion as that of the existence of qualitative differences in permanent electric currents, according as few or many voltaic elements are concerned in their formation, or of periodicity as a *necessary* condition of a voltaic current, if fully established, cannot be overrated, the conclusion does not seem to be fairly deducible from the experiments described. It would rather seem that, from some cause yet to be investigated, the motion of the mercurial cathode, or rather the change of figure resulting from the motion, alters the total electromotive force or resistance (more probably the resistance) in the circuit, and thus, by altering the current, reacts upon the forces whereby the motion of the cathode is produced. In a circuit of small resistance, it might be expected according to this view that a smaller motion of the cathode would suffice to bring about a given change in the current, and a corresponding change in the force pro-

* I employ the word "quantity" in its ordinary sense, viz. as that indicated by measurement of gases from decomposition of water in a voltameter.

ducing the motion, and accordingly that the period of the changes would be shorter than in a circuit of greater resistance, although the *mean* currents in the two circuits, as measured by a galvanometer or voltameter, might be the same.]

“On the Diurnal Tides of Port Leopold, North Somerset.” By the Rev. Samuel Haughton, M.A., F.R.S.

“On the Posterior Lobes of the Cerebrum of the *Quadrupana*.” By William Henry Flower, Esq., F.R.C.S.

“On the General Forms of the Symmetrical Properties of Plane Triangles.” By Thomas Dobson, Esq., B.A.

“Note on Ethylene-Dichloride of Platinum.” By P. Griess, Esq., and C. A. Martins, Ph.D.

January 16, 1862.—Dr. William Allen Miller, Treasurer and Vice-President, in the Chair

The following communications were read:—

“On the Development of Striped Muscular Fibre in Man, Mammalia, and Birds.” By J. Lockhart Clarke, Esq., F.R.S.

“On the Influence of Temperature on the Electric Conducting Power of the Metals.” By A. Matthiessen, Esq., F.R.S., and M. von Bose.

In the first part of the paper we have described the apparatus used for the experiments, together with the precautions taken to ensure correct results; in the second we have given the results obtained with the pure metals—silver, copper, gold, zinc, tin, arsenic, antimony, bismuth, mercury—and the metalloid tellurium. The conducting power of the wires, or bars of each, was determined at about 12° , 25° , 40° , 55° , 70° , 85° , and 100° C.; and from the mean of the eight observations made with each wire (four at each temperature on heating, and four on cooling), we deduced a formula by the method of least squares for the correction of the conducting power for temperature. It was found that the conducting power or resistance of a metal does not decrease or increase in direct ratio to the temperature, as stated by Becquerel*, Arndsten†, and Siemens‡, who assume that the formula for the correction of resistance for temperature between 0° – 100° may be expressed by

$$\lambda = x + yt,$$

but that, on the contrary, the formula must be

$$\lambda = x + yt + \gamma t^2,$$

where λ is the resistance at t degrees, x the resistance at 0° , and y and γ constants. One fact seems to have escaped the observation of former experimenters, namely, that when a wire of a metal is

* Ann. de Chim. et de Phys. (3) xvii. 242.

† Pogg. Ann. civ. 1.

‡ Pogg. Ann. cxiii. 91.

heated for the first time to 100° and again cooled, an alteration in the conducting power takes place; with most metals it is necessary to heat them for several days before their conducting power becomes constant. In the third part we have deduced from the results obtained, the law that *all pure metals in a solid state vary in conducting power to the same extent between 0° and 100° C.* In cases where very great accuracy is required, it is absolutely necessary to experiment on the conductor itself; for we have found almost the same differences between formulæ obtained for wires of the same metal as between the mean of those deduced for the different metals. This behaviour may be attributed to the fact that the molecular arrangement is not the same even in wires of the same metal; for we find that copper wires, when kept at 100° for several days, behave very differently from each other: thus, in the case of the three copper wires experimented with, wire 1 increased in conducting power almost to the same extent as if it had been annealed, wire 2 partially so, and wire 3 hardly at all. With bismuth, wire 1 increased its conducting power 16 per cent.; wire 2, 19 per cent.; and wire 3, 12 per cent. Again, in the case of cadmium, which becomes quite brittle and crystalline at 80° (for cadmium may be powdered in a hot mortar), we found the formula for each wire very different. On the other hand, the formulæ of the wires of those metals which, after being kept at 100° for some time, show a very slight or no alteration in the conducting power on again being cooled, agree very closely with each other. Compare those of lead, tin, and mercury.

Metalloids conduct electricity better when heated than when cold. Hittorf* proved this to be the case with selenium. Gas-coke and graphite†, and the gases‡, follow the same law. Tellurium, when first heated to 70° or 80° C., behaves as a metal, that is to say, it loses in conducting power up to that temperature, when it then begins to gain. The temperature of the turning-point becomes lower after each day's heating, until, as with the first and third bars experimented with, it is below the lowest temperature at which observations were made. Taking the first observed conducting power of each bar = 100, we found that the conducting power of bar 1 had decreased after thirteen days' heating to 4, where it then remained constant; that of bar 2, after thirty-two days, became constant at 19; and that of bar 3, after thirty-three days, at 6. With bar 2 the conducting power decreased up to $29^{\circ}4$, when it began again to increase. The behaviour of tellurium is therefore intermediate between that of the metals and that of the metalloids.

“Notes of Researches on the Poly-Ammonias.”—No. XIX. On Aromatic Diamines. By A. W. Hofmann, LL.D., F.R.S.

* Pogg. Ann. lxxxvi. 214.

† Phil. Trans. 1858, p. 586.

‡ Ann. de Chim. et de Phys. (3) xxxix. 355.

LIV. *Intelligence and Miscellaneous Articles.*

ON THE COMBUSTION OF GUNPOWDER *in vacuo*, AND IN VARIOUS GASEOUS MEDIA. BY M. BIANCHI.

I HAVE the honour to lay before the Academy a *résumé* of the principal results of my experiments on the combustion of powder in air, and in various gaseous media.

The apparatus I have used consists of a glass globe, in which, by a socket and by means of a screw-thread, a support penetrates, which can be adapted to the air-pump. This support is traversed by two metallic rods with clamps, well insulated and intended to receive a small crucible formed of platinum wire about half a millimetre in diameter, rolled in a conical spiral and closed by a cover, also formed of platinum wire. In this crucible the powder is placed. By means of a battery of three or four Bunsen's elements, the platinum wire which constitutes the crucible is heated to redness. The principal facts observed may be summed up in the following phenomena:—

1. Ordinary powder, fulminates, and all gunpowders, in grains or in compact masses, placed freely *in vacuo*, that is to say, in a great space as compared with the volume of the powder, and submitted to the sudden action of a heat of more than 2000° , burn slowly and without producing, as in air, a brisk deflagration.

2. On the contrary, when the powder is enclosed in a pistol and also exhausted, and is ignited by means of a platinum wire raised to redness, or, better, by a percussion cap, it explodes as rapidly as in air.

3. *In vacuo* the combustion of gun-cotton takes place slowly in successive layers, commencing by the parts nearest the source of heat; once commenced, it continues until the gun-cotton completely disappears, without its being in contact with the incandescent focus; lastly, this combustion takes place without producing light, even in the greatest darkness.

4. The products of combustion are not the same as in air.

5. The combustion of powder takes place in nitrogen, carbonic acid, and other gaseous media unfit for combustion, with a rapidity and briskness almost equal to that in air.—*Comptes Rendus*, July 14, 1862.

OBSERVATIONS ON THE SOLAR SPECTRUM. BY DR. A. WEISS.

In former years I have repeatedly observed the condensation of Fraunhofer's lines in the solar spectrum; and a communication of mine on this subject appears in the *Philosophical Magazine*, vol. xxii. p. 80. I thought even then that I had discovered an analogy with certain absorptions which I had observed in other spectra, and I am glad to have had opportunity of confirming this view.

In a journey to Greece in last year, in which I was provided with a Soleil's spectroscope, I observed from the vessel as often as I could

the sunrise and sunset on the sea ; for, from the purity of the Ionian sky, sharp pictures were to be expected even on the horizon.

In fact I succeeded in observing, and often in the most surprising manner, the phenomenon of condensation, especially of the lines in the red and yellow of the spectrum, which I had seen in our latitudes, although much more dimly. I also observed the increase in the number of the lines at the setting of the sun. A single glance into the spectroscope was enough to convince even non-scientific spectators, and hence to remove any doubt as to a possible subjective perception. I have repeated these observations not only on the sea, but also on the continent, in Athens, in Zante, Cerigo, and Ithaca ; and although no measurements could be made, the phenomena were so marked, that, from the relative position of the spectral lines, I can almost guarantee in this case a one-sided condensation towards the violet end, just as I have shown it in the case of hyponitrous acid and chlorophyll*.

Forspectrum observations, Greece, and especially the Ionian Islands, are most suitable stations ; for the dense layers of vapour observed almost every day on the coasts of Africa and America, there scarcely disturb the observer.—Poggendorff's *Annalen*, May 1862.

MODE OF APPLYING THE ELECTRIC LIGHT FOR MINING PURPOSES.

BY MM. DUMAS AND BENOIT.

The apparatus consists essentially of three parts—a battery, a Ruhmkorff's coil, and a Geissler's tube—the whole arranged so as to produce a sufficient light to illuminate the miner, and allow him to work in atmospheres where other lights fail.

The light produced is cold, or rather does not heat the tube in which it is produced ; and gas has no access to it : it is quite isolated. The apparatus is as compact as ordinary lamps, and there is no injurious emanation. It can be lighted or extinguished at will. It can work for twelve consecutive hours without diminution, and without requiring any change. The workman has only occasionally to agitate the carbon by means of a rod.

The greatest difficulty consisted in being able to associate a battery of such intensity that the weight of the apparatus was as small as possible, the light produced of the greatest regularity, and its duration at least twelve hours. The present form of the apparatus, which may be still further diminished, is already so small that the miner can carry it without inconvenience, like a small carpet bag.

The authors point out the advantages of such a mode of illumination, and state that the results obtained in using Becquerel's fluorescence-tubes have led to the expectation that the luminous effects may be greatly improved both as to duration and intensity.—*Comptes Rendus*, September 8, 1862.

* Phil. Mag. vol. xxii. p. 80.

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[FOURTH SERIES.]

DECEMBER 1862.

LV. *Tests of the Truth of the Fluid Theory of the Figure of the Earth.* By Archdeacon J. H. PRATT*.

1. **T**HE calculation of the figure of the earth on the fluid theory proceeds upon the hypothesis that the earth at one time consisted of a heterogeneous mass of incompressible fluid or semifluid materials revolving round a fixed axis, and that these materials arranged themselves in layers nearly spherical in form ere the mass, or at any rate its surface, became solid as at present. The law of density of the layers is assumed to be represented by $A \frac{\sin qa}{a}$, a being the mean radius of any layer, and A and q constants. Though this is an assumed law, it has not been chosen at random, but has been selected for good reasons.

2. The following are some of the results flowing from this hypothesis. The layers are all spheroidal, with a common centre and axis; their ellipticities are related by the equation

$$\frac{\epsilon'}{\epsilon} = \frac{\frac{3}{q^2 a^2} - \frac{1}{z'}}{\frac{3}{q^2 a^2} - \frac{1}{z}},$$

ϵ and a being the ellipticity and semi-axis major of the surface, and ϵ' and a the same of any other layer; also

$$z = 1 - \frac{qa}{\tan qa}, \quad z' = 1 - \frac{qa}{\tan qa} \dagger.$$

* Communicated by the Author.

† These formulæ may be gathered from any book on this subject. I have derived them from my treatise 'On the Figure of the Earth,' second edition, p. 76.

Phil. Mag. S. 4. Vol. 24. No. 163. Dec. 1862.

2 E

By making $ga = 140^\circ 45'$, $= 2.4576$ in arc, the theory gives the ellipticity of the surface equal to $\frac{1}{292}$ *, which is remarkably near $\frac{1}{294}$ and $\frac{1}{290}$, the last determination by geodesy and pendulum experiments. (See Preface to the volume of the 'British Ordnance Survey,' and p. 453 of Humboldt's 'Cosmos,' vol. iv. part 1.)

3. My present object is to test the general correctness of the distribution of the earth's mass which the fluid theory leads to, by showing what effect will be produced upon the formula for the variation of gravity, and therefore upon the computed number of vibrations of a seconds' pendulum, by a change in the arrangement of the materials. Problems of this description are of such difficulty, that I am of course very much restricted in selecting a readjustment so as to make the calculation possible. If the earth has not derived its figure from having been once fluid, we know no more of its internal structure than that its mean density is twice that of the superficial parts; that the average arrangement of the materials must be pretty uniform around the centre, as gravity in all parts of the world tends nearly towards the geometrical centre of the earth; and that the increase of gravity in passing from one place to another changes very much as the square of the sine of the latitude does. The arrangements which I suggest as tests are, under these circumstances, as likely to be true, *as to their resultant effect upon the surface*, as any other arrangement which might be tried.

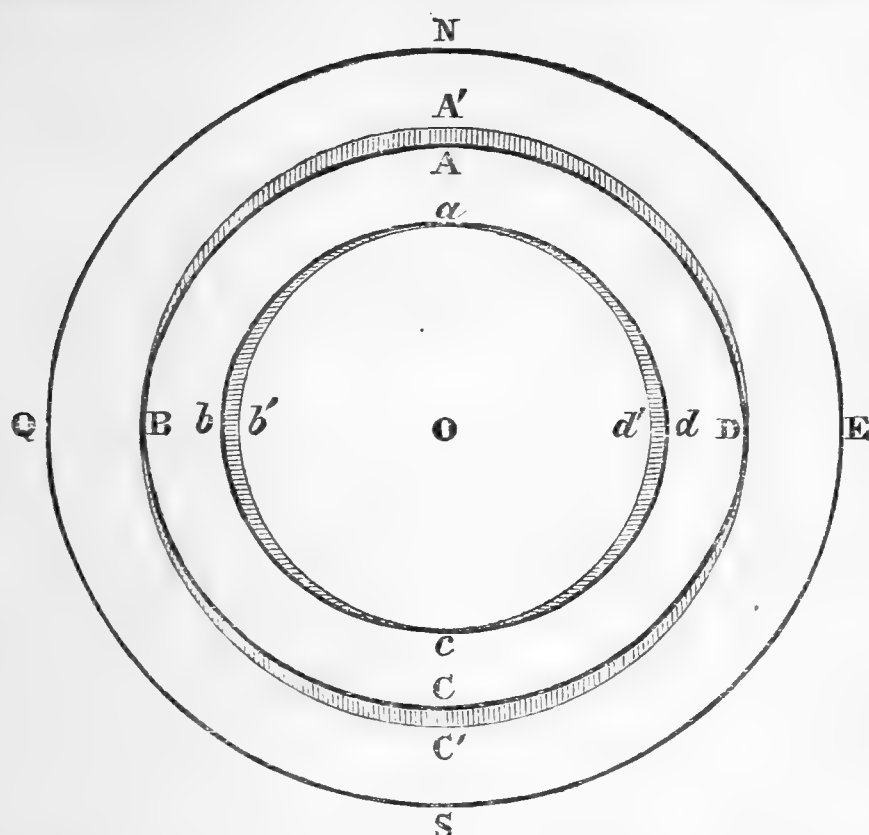
First test: the ellipticity of some of the strata changed.

4. The diagram represents a section of the earth's mass through the poles N and S. A B C D and a b c d are two layers or spheroidal surfaces bounding a shell of the mass, of one-fifth of the radius in thickness, that is, 800 miles. The first change I imagine to take place is this. All the strata of this shell lose their ellipticity and become spherical; at A and C the mass swells up, as it were, and increases the density of the inner parts of the outer crust in A A' and C C'; and at b and d the mass penetrates the inner nucleus as far as b' and d', A' B C' D and a b' c d' being circles, and the other curves oblate ellipses†. In this way the total mass is not altered; but the mass belonging to the shell no longer attracts as a spheroidal shell, but as a mass consisting only of spherical layers, which may therefore be considered as concentrated in the centre of the earth. This change

* On the Figure of the Earth, second edition, p. 78.

† A A' and C C', as well as b b' and d d', are drawn too large in the diagram. A A' should be about $\frac{1}{360}$ th of O N, and b b' should be about $\frac{1}{460}$ th part.

in the arrangement of the materials is in reality very small. It simply supposes the density in the thin curved layers $A A'$ and



$C C'$, as well as $b b'$ and $d d'$, to be doubled, and the excess of matter for this purpose to be drawn from the whole of the shell by a very trifling attenuation.

5. I shall take four examples of depth below the surface; viz. four, three, two, and one fifths of the radius. The following will be the values of the several quantities to be used, and the results which the formula of paragraph 2 leads to:—

Surface.	Second ellipse.	Third ellipse.	Fourth ellipse.	Fifth ellipse.
$qa = 140^\circ 45'$	$112^\circ 36'$	$84^\circ 27'$	$56^\circ 18'$	$28^\circ 9'$
or 2.458	1.966	1.475	0.983	0.492
$z' = 4.008$	1.818	0.857	0.344	0.081
$\frac{\epsilon'}{\epsilon} = 1$	0.915	0.859	0.814	0.540
$\frac{E'}{E} = 1$	0.663	0.337	0.113	0.015

E and E' are the masses of the whole earth and of that part of it which lies within the outer surface of the shell which is under consideration. The above values of E' are the approximate values, neglecting the ellipticity of the strata, because E' will be used only in small terms: it is calculated from the formula

$$E' = \int_0^a 4\pi a^2 \cdot A \frac{\sin qa}{a} da = \frac{4\pi A}{q^2} \sin qa \left(1 - \frac{qa}{\tan qa} \right).$$

2 E 2

6. I proceed now to find the effect of the change in distribution upon the formula for gravity. Let V , V_1 , and V_2 be the potentials for the whole mass of the earth and for the spheroidal portions of it lying within the outer and inner surfaces of the shell. Then*

$$\begin{aligned} V &= \frac{E}{r} + \left(\epsilon - \frac{m}{2}\right) \frac{Ea^2}{r^3} \left(\frac{1}{3} - \mu^2\right), \\ V_1 &= \frac{E_1}{r} + \left(\epsilon_1 - \frac{m_1}{2}\right) \frac{E_1 a_1^2}{r^3} \left(\frac{1}{3} - \mu^2\right), \\ V_2 &= \frac{E_2}{r} + \left(\epsilon_2 - \frac{m_2}{2}\right) \frac{E_2 a_2^2}{r^3} \left(\frac{1}{3} - \mu^2\right). \end{aligned}$$

μ = sine of latitude, also m = ratio of centrifugal force to gravity at the equator $= \frac{1}{289}$;

$$\therefore \frac{m_1}{m} = \frac{E}{E_1} \frac{a_1^3}{a^3}, \quad \frac{m_2}{m} = \frac{E}{E_2} \frac{a_2^3}{a^3}.$$

Now $V_1 - V_2$ is the potential of the shell: this must be replaced by the potential of the same mass as it is differently distributed, viz. by $(E_1 - E_2) \div r$. Let U be the potential of the whole earth under the new distribution; then

$$\begin{aligned} U &= V - (V_1 - V_2) + (E_1 - E_2) \div r \\ &= \frac{E}{r} + \frac{Ea^2}{r^3} \left\{ \epsilon - \frac{m}{2} - \left(\epsilon_1 - \frac{m_1}{2}\right) \frac{E_1}{E} \frac{a_1^2}{a^2} + \left(\epsilon_2 - \frac{m_2}{2}\right) \frac{E_2}{E} \frac{a_2^2}{a^2} \right\} \left(\frac{1}{3} - \mu^2\right) \\ &= \frac{E}{r} + \frac{Ea^2}{r^3} \left\{ \epsilon - \frac{m}{2} - \epsilon_1 \frac{E_1}{E} \frac{a_1^2}{a^2} + \frac{m}{2} \frac{a_1^5}{a^5} + \epsilon_2 \frac{E_2}{E} \frac{a_2^2}{a^2} - \frac{m}{2} \frac{a_2^5}{a^5} \right\} \left(\frac{1}{3} - \mu^2\right) \\ &= \frac{E}{r} + \frac{Ea^2}{r^3} \left\{ \epsilon - \frac{m}{2} - G \right\} \left(\frac{1}{3} - \mu^2\right) \text{ suppose.} \end{aligned}$$

Let g and g' be the value of gravity before and after the change in distribution,

$$\therefore g = -\frac{dV}{dr} - \text{cent. force} = -\frac{dV}{dr} - m \frac{E}{a^2} (1 - \mu^2),$$

$$g' = -\frac{dU}{dr} - m \frac{E}{a^2} (1 - \mu^2);$$

$$\therefore \frac{g'}{g} = \frac{1 + 3\left(\epsilon - \frac{m}{2} - G\right)\left(\frac{1}{3} - \mu^2\right) - m(1 - \mu^2)}{1 + 3\left(\epsilon - \frac{m}{2}\right)\left(\frac{1}{3} - \mu^2\right) - m(1 - \mu^2)},$$

* See 'Figure of the Earth,' p. 93.

$=1-3G\left(\frac{1}{3}-\mu^2\right)$, neglecting small quantities of the second order.

Hence $3G$ is the quantity which measures the ratio in which gravity is diminished between the equator and the poles by this change of distribution.

7. Suppose the earth's mass to be divided into five portions, viz. four shells each 800 miles thick, and a central nucleus of 800 miles radius. I will proceed to find the numerical value of $3G$ for each of the three shells between the outer shell and the central nucleus.

$$G=\epsilon_1\frac{E_1}{E}\left(\frac{a_1}{a}\right)^2-\epsilon_2\frac{E_2}{E}\left(\frac{a_2}{a}\right)^2-\frac{m}{2}\frac{a_1^5-a_2^5}{a^5}.$$

Substituting in this the values found in paragraph 5, and putting $\epsilon=\frac{1}{294}$ (the value given in the volume of the British

Ordnance Survey) and $m=\frac{1}{289}$, we have for the first shell

$G=0\cdot00053$, for the second shell $G=0\cdot00018$, for the third shell $G=0\cdot00003$. Thus the three values of $3G$ in these three cases are $0\cdot00159$, $0\cdot00054$, and $0\cdot00009$.

The increase of gravity, in the actual state of the earth, in passing from the equator to the poles is $0\cdot00518^*$. Hence the change of distribution of the materials in the above three cases would produce a decrease of gravity in passing from the equator to the poles, bearing the following ratios to the actual and known increase of gravity:—

$$\begin{array}{ccc} 0\cdot00159 & 0\cdot00054 & 0\cdot00009 \\ \hline 0\cdot00518' & 0\cdot00518' & 0\cdot00518' \\ \text{or } \frac{3}{10}\text{ths,} & \frac{1}{10}\text{th,} & \text{and } \frac{1}{60}\text{th.} \end{array}$$

The highest northern station at which pendulum experiments have been made is Spitzbergen. There 214 more vibrations are made by a seconds' pendulum in 24 hours than at the equator†. In the three cases under consideration these would be diminished by 64, 21, and $3\frac{1}{2}$ vibrations in the course of the day. The first and second of these would be very easily detected, though perhaps not the third, at so great a distance as Spitzbergen is from the equator, considering the difficulty of comparing the pendulums at the two places or transferring the pendulum from one place to the other without injury. The first and second, however, fully answer my purpose. They show how sensible a change would be produced in the variation of gravity by the slight alteration which I have suggested as a first test.

* See Humboldt's 'Cosmos,' vol. iv. part 1, p. 468.

† Ibid. p. 469.

Second test: the density of the strata supposed to be interchanged, but their form to remain unaltered.

8. As a second test, I will suppose the density of one of the shells, as for example the second, to be altered throughout in the ratio $\alpha : 1$, and that of the third to be altered in the ratio $\beta : 1$, in such a way as not to affect the total mass. This leads to the condition

$$\alpha(E_1 - E_2) + \beta(E_2 - E_3) = E_1 - E_3;$$

and the potential of the whole mass is

$$\begin{aligned} U &= V - V_1 + \alpha(V_1 - V_2) + \beta(V_2 - V_3) + V_3, \\ &= \frac{E}{r} + \frac{Ea^2}{r^3} \left\{ \epsilon - \frac{m}{2} + (\alpha - 1) \frac{E_1}{E} \frac{a_1^2}{a^2} \left(\epsilon_1 - \frac{m_1}{2} \right) \right. \\ &\quad \left. + (\beta - \alpha) \frac{E_2}{E} \frac{a_2^2}{a^2} \left(\epsilon_2 - \frac{m_2}{2} \right) + (1 - \beta) \frac{E_3}{E} \frac{a_3^2}{a^2} \left(\epsilon_3 - \frac{m_3}{2} \right) \right\} \left(\frac{1}{3} - \mu^2 \right), \\ &= \frac{E}{r} + \frac{Ea^2}{r^3} \left\{ \epsilon - \frac{m}{2} + (\alpha - 1) \left(\frac{E_1}{E} \frac{a_1^2}{a^2} \epsilon_1 - \frac{m}{2} \frac{a_1^5}{a^5} \right) \right. \\ &\quad \left. + (\beta - \alpha) \left(\frac{E_2}{E} \frac{a_2^2}{a^2} \epsilon_2 - \frac{m}{2} \frac{a_2^5}{a^5} \right) + (1 - \beta) \left(\frac{E_3}{E} \frac{a_3^2}{a^2} \epsilon_3 - \frac{m}{2} \frac{a_3^5}{a^5} \right) \right\} \left(\frac{1}{3} - \mu^2 \right). \end{aligned}$$

Now, by the relation connecting α and β ,

$$\beta - \alpha = -(\alpha - 1) \frac{E_1 - E_3}{E_2 - E_3}, \quad 1 - \beta = (\alpha - 1) \frac{E_1 - E_2}{E_2 - E_3}.$$

If, then, we put

$$\begin{aligned} U &= \frac{E}{r} + \frac{Ea^2}{r^3} \left\{ \epsilon - \frac{m}{2} + H \right\} \left(\frac{1}{3} - \mu^2 \right), \\ H &= (\alpha - 1) \left\{ \frac{E_1}{E} \frac{a_1^2}{a^2} \epsilon_1 - \frac{m}{2} \frac{a_1^5}{a^5} - \frac{E_1 - E_3}{E_2 - E_3} \left(\frac{E_2}{E} \frac{a_2^2}{a^2} \epsilon_2 - \frac{m}{2} \frac{a_2^5}{a^5} \right) \right. \\ &\quad \left. + \frac{E_1 - E_2}{E_2 - E_3} \left(\frac{E_3}{E} \frac{a_3^2}{a^2} \epsilon_3 - \frac{m}{2} \frac{a_3^5}{a^5} \right) \right\}; \end{aligned}$$

and, precisely as in the other case, the increase of gravity in passing from the equator to the poles $= 3H$.

9. This I will now reduce to numbers. By substituting from paragraph 5,

$$\begin{aligned} H &= \frac{\alpha - 1}{294} \left(0.38825 - \frac{550}{224} \times 0.10422 + \frac{326}{224} \times 0.01472 \right) \\ &\quad - \frac{\alpha - 1}{578} \left(0.32768 - \frac{550}{224} \times 0.07776 + \frac{326}{224} \times 0.01024 \right), \\ &= (\alpha - 1)(0.000523 - 0.000262) = (\alpha - 1) \times 0.00026. \end{aligned}$$

Hence $3H = (\alpha - 1) \times 0.00078$, and this change in the distribu-

tion of the mass will produce an increase in gravity in passing from the equator to the poles bearing the following ratio to the increase actually observed,

$$(\alpha-1) \frac{0.00078}{0.00518} \text{ or } \frac{3}{20} (\alpha-1),$$

and the number of vibrations lost at Spitzbergen by a seconds' pendulum taken there from the equator, arising from this cause, would be $32(\alpha-1)$.

By the formula already deduced,

$$1-\beta = (\alpha-1) \frac{E_1-E_2}{E_2-E_3} = (\alpha-1) \frac{326}{224} = \frac{3}{2} (\alpha-1) \text{ nearly.}$$

Hence, for any change made in the density of the upper shell, a change half as great again must be made in the lower one to preserve the whole mass constant. Also the ratio of the mean densities of the shells when altered will be

$$\begin{aligned} &= \frac{\text{mass of upper shell}}{\text{mass of lower shell}} \times \frac{\text{volume of lower shell}}{\text{volume of upper shell}} \\ &= \frac{\alpha(E_1-E_2)}{\beta(E_2-E_3)} \times \frac{27-8}{64-27} = \frac{3\alpha}{4\beta} \text{ nearly.} \end{aligned}$$

Also, from above,

$$\alpha = \frac{5}{3} - \frac{2}{3} \beta.$$

Suppose now that the mean densities of the two shells are made the same; then $\beta = \frac{3}{4}\alpha$, and $\alpha = \frac{10}{9}$, $\beta = \frac{5}{6}$, or the upper shell is increased in density $\frac{1}{9}$ th, and the lower diminished $\frac{1}{6}$ th; and the number of seconds lost on this account, by removing the seconds' pendulum from the equator to Spitzbergen $= 3\frac{1}{2}$, a quantity perhaps hardly to be detected with certainty.

If the mean density of the upper shell is made twice as great as that of the lower, then $\beta = \frac{3}{8}\alpha$, $\alpha = \frac{4}{3}$, $\beta = \frac{1}{2}$; or the upper density is increased $\frac{1}{3}$ rd, and the lower diminished by one-half; and the seconds lost at Spitzbergen would be 11, which might readily be detected.

Suppose that the density of the lower shell is diminished indefinitely so as to make it practically a vast cavern, and its mass to be gathered into the upper shell. Then β is extremely small, $\alpha = \frac{5}{3}$, and the seconds lost at Spitzbergen would be 21.

10. It will thus be seen from this second test, that the change in distribution of matter relatively to the centre would not have so

much effect on the variation of gravity as the change of the strata in form, that is, relatively to the axis. This, indeed, is what might have been anticipated; and it tells, I think, very greatly in favour of the fluid theory. I can conceive it possible that a crude irregular solid mass revolving round an axis might in the course of indefinite ages assume a more or less external spheroidal form. For the perpetual degradation of the solid prominences by the process of weathering would set free particles which with the fluid portions would find a position on the surface approaching to conformity with the laws of the equilibrium of fluids. But no process of this kind, or any other process, could make the interior parts bulge out towards the equator in strata more or less spheroidal with the axis of revolution as a common axis, unless those parts were moveable like a fluid or semifluid mass.

That the earth's mass consists of nearly spherical strata, is not dependent on any theory.

11. Before concluding, I would remark that it has, I think, generally been assumed that the notion that the earth's mass consists of strata more or less spherical about the earth's centre is a deduction from the fluid theory, and that, apart from that theory, we have no knowledge whatever regarding the distribution of the matter in the interior of the earth. This, as I have intimated in paragraph 3, is hardly correct.

There are two facts regarding the law of gravity at the surface of the earth which observation has brought to light: (1) that the plumb-line points in all parts of the world very nearly to the geometrical centre of the globe; and (2) that the amount of gravity is very nearly constant all over the globe, slightly increasing from the equator towards the poles, but by such small quantities that at Spitzbergen, in about latitude 80° , the increase is only about $\frac{1}{200}$ th part of that at the equator. Now, if it were true that at two or three places only the plumb-line pointed nearly in the direction of the diameters of the earth at those places, we could not infer anything more than that the matter of the earth is distributed with reference to those particular diameters in such a way as to produce no horizontal effect at the surface. But since this is nearly the case with *every* diameter, as observation teaches us, no other conclusion is possible but that the matter of the earth is arranged nearly symmetrically about every diameter, and therefore about the centre of the earth. For, suppose there is a sensible preponderance of matter on one side of the centre above that on the opposite side. The plumb-line at the two places at the extremities of the diameter passing through the midst of that preponderance may hang towards the centre of the earth; but at places away from those two particular spots, the plumb-line would be swayed away from

the centre towards the side where the preponderance is. This is in no case found to be true, very minute quantities being neglected. The amount of gravity, moreover, at the two spots above described would not be the same, although they would be in the same latitude, one north and the other south. But neither is this result found to be a fact in nature. The only change perceived in the amount of gravity is a very slight increase in proceeding northwards from the equator, an effect which accords precisely with another observed fact, that the number of miles in a degree of latitude increases in proceeding northwards, and therefore the globe is somewhat flattened towards the poles, which would produce this very effect of an increase in gravity, as the ordinary principles of attraction show. I think it is clear, then, apart from all theory, that the mass of the earth is made up of nearly spherical strata arranged about the earth's centre. The law of density of these strata we have no means of determining by this kind of reasoning, nor of ascertaining even whether the strata first increase in density and then decrease in approaching the centre, or increase the whole way. It is only the fluid theory which can help us here.

I would observe that this arrangement of the earth's mass is assumed, avowedly or tacitly, in every calculation I have seen on the subject. Thus, in Professor Stokes's valuable paper "On the Variation of Gravity" (Cambridge Philosophical Transactions, 1849), he assumes it in the step where he states that the series

$$V = \frac{Y_0}{r} + \frac{Y_1}{r^2} + \dots$$

would reduce itself to its first term, if the surface were spherical and the earth had no motion of rotation. This would not be the case unless the sphere were homogeneous, or consisted of concentric spherical shells of matter; for in that case only does the potential of a sphere for an external point vary inversely as the distance from the centre.

Calcutta, October 1, 1862.

LVI. On the Violet Flame of many Chlorides.

To the Editors of the *Philosophical Magazine and Journal*.

GENTLEMEN,

IN a recent Number of Poggendorff's *Annalen* there is an important paper by Alexander Mitscherlich, in which he shows that the prismatic spectra of the flames of certain compounds of the metals are different from those of the metals themselves. He gives drawings of the spectra obtained from four chlorides

and one iodide, and shows the bearing of his observations on solar chemistry and other questions.

Now his experiments, though very valuable and suggestive, are far from giving a complete story; and they recalled to my recollection some experiments of my own, the record of which had never proceeded further than my note-book, but which evidently were elucidated by, and were capable of elucidating those of the German physicist. I have repeated and extended these during the last few days, and I send you the results, hoping to lead other experimenters into the same field, and perhaps to return to it myself when I have greater leisure.

The fact is that the majority of the lines represented in Alexander Mitscherlich's diagram of the flame of chloride of copper, and which, by the way, are represented in Prof. W. A. Miller's diagram of the same in the *Philosophical Magazine* for August 1845, are common to a large number of chlorides if they are sufficiently heated. In the whole range of spectrum-analysis there is no fact better known than that common salt gives rise to the yellow double line D; yet it is popularly known that if salt be thrown on to the red-hot coals of an ordinary fire it produces violet flames. This is beautifully seen when old ship timber is burnt. These flames, when analysed, are found to consist, not of the yellow light, but of three groups of lines; the first green, and extending to the fixed line *b*; the second bluish green, and blue, lying on either side of F; and the third violet, stretching from midway between F and G to a little beyond G. When examined by a spectroscope of no great power, these three groups have a close resemblance to one another, each consisting of four lines about equidistant, of which the two middle ones are brighter than the outer ones; but when they are more carefully examined with a narrow slit, the lines of the second and third groups at least appear as bands of a certain width, and are even resolvable into double bands, of which the more refrangible are the narrower and fainter. That they are identical with the lines of chloride of copper was proved, not only by the identity of their appearance, but by angular measurement of the more prominent ones. No more refrangible rays are seen when this light is examined by a spectroscope the lenses and prism of which are made of quartz.

Many other chlorides also give the same light when sufficiently heated. The flame of a spirit-lamp is enough to produce it with the chloride of copper; and the chloride of platinum or of gold, if put into such a flame, gives a bright momentary flash of the violet light, which, when analysed by the prism, is resolved into the same bands, those from the platinum-salt exactly coinciding with those from the copper salt when the two are seen side

by side. Chloride of mercury in the gas flame from a Bunsen's burner gives this light also. A hydrogen flame is capable of producing it from the chlorides of nickel and cobalt; but it requires the heat of red-hot coals to obtain it from chemically pure chloride of sodium, potassium, or barium; indeed in the latter case the fire must be very intense, but there is then no mistaking the characteristic violet. Chloride of zinc also exhibited it at this high temperature, and so did chloride of iron, though less distinctly; but a doubt must rest on such observations made with a coal-fire, as it is quite conceivable that these chlorides may give up their chlorine to the alkalies or earths of the ash. Chloride of silver was doubtful; and I did not succeed in obtaining the violet colour from either chloride of calcium, lead, or manganese.

That this light should be emitted from chemically pure chloride of barium at a very high temperature has a peculiar interest, since it was this chloride which first attracted Alexander Mitscherlich's notice by green lines not belonging to barium, that make their appearance when it is ignited in association with chloride of ammonium. These green lines do not coincide with those of the violet flame.

The question naturally arises, to what is this violet flame due? Is it the chloride itself in the gaseous form that emits these rays when heated up to a certain temperature, which differs according to the metal with which the chlorine is combined? Or is the peculiar flame produced by chlorine when the chloride is decomposed by heat? Or does it depend on the combination of the chlorine with the carbon or hydrogen of the combustible? The latter supposition is negatived by the fact that anhydrous chloride of copper emits these rays equally whether it be placed in a flame of hydrogen or of pure bisulphide of carbon. It is difficult to accept the second supposition; for though chloride of copper or gold is certainly decomposed in the spirit-lamp flame, chloride of nickel or iron is so likewise, and chloride of mercury is reduced to the subchloride, and yet these last do not exhibit the coloured flame at that temperature. Besides, a stream of chlorine or of hydrochloric acid passed into a flame never gives the violet light; nor does Dutch liquid, muriatic ether, or chloroform mixed with alcohol and burnt in a spirit-lamp. The rays in question also appear to bear no relation to those emitted in Plücker's experiments. The origin of this violet light evidently, therefore, requires further elucidation.

I remain, Gentlemen,

Yours faithfully,

J. H. GLADSTONE.

28 Pembridge Gardens,
November 15, 1862.

LVII. *On the Exact Form and Motion of Waves at and near the Surface of Deep Water.* By WILLIAM JOHN MACQUORN RANKINE, C.E., LL.D., F.R.S.S.L. & E. &c.*

THE following is a summary of the nature and results of a mathematical investigation, the details of which have been communicated to the Royal Society.

The investigations of the Astronomer Royal and of Mr. Stokes on the question of straight-crested parallel waves in a liquid are based on the supposition that the displacements of the particles are small compared with the length of a wave. Hence it has been very generally inferred that the results of those investigations, when applied to waves in which the displacements are considerable as compared with the length of wave, are only approximate.

In the present paper the author proves that one of those results—viz. that in very deep water the particles move with a uniform angular velocity in vertical circles, whose radii diminish in geometrical progression with increased depth, and consequently that surfaces of continuity and equal pressure, including the upper surface, are trochoidal—is exact for all possible displacements, how great soever.

The trochoidal form of waves was first explicitly described by Mr. Scott Russell; but no demonstration of its exactly fulfilling the cinemactical and dynamical conditions of the question has yet been published, so far as the author knows.

In ‘A Manual of Applied Mechanics’ (first published in 1858), the author stated that the theory of rolling waves might be deduced from that of the positions assumed by the surface of a mass of water revolving in a vertical plane about a horizontal axis; but as the theory of such waves was foreign to the subject of the book, he deferred until now the publication of the investigation on which that statement was founded.

Having communicated some of the leading principles of that investigation to Mr. William Froude in April 1862, the author was informed by that gentleman that he had arrived independently at similar results by a similar process, although he had not published them. The introduction of Prop. II. between Props. I. and III. is due to a suggestion by Mr. Froude.

The following is a summary of the leading results demonstrated in the paper.

Proposition I.—In a mass of gravitating liquid whose particles revolve uniformly in vertical circles, a wavy surface of trochoidal profile fulfils the conditions of uniformity of pressure,—such

* Communicated by the Author, being an abstract of an investigation read before the British Association, 1862, Section A.

trochoidal profile being generated by rolling, on the under side of a horizontal straight line, a circle whose radius is equal to the height of a conical pendulum that revolves in the same period with the particle of liquid.

Proposition II.—Let another surface of uniform pressure be conceived to exist indefinitely near to the first surface: then if the first surface is a surface of continuity (that is, a surface always traversing identical particles), so also is the second surface.

(Those surfaces contain between them a continuous layer of liquid.)

Corollary.—The surfaces of uniform pressure are identical with surfaces of continuity throughout the whole mass of liquid.

Proposition III.—The profile of the lower surface of the layer referred to in Prop. II. is a trochoid generated by a rolling circle of the same radius with that which generates the upper surface; and the tracing-arm of the second trochoid is shorter than that of the first trochoid by a quantity bearing the same proportion to the depth of the centre of the second rolling circle below the centre of the first rolling circle, which the tracing-arm of the first rolling circle bears to the radius of that circle.

Corollaries.—The profiles of the surfaces of uniform pressure and of continuity form an indefinite series of trochoids, described by equal rolling circles, rolling with equal speed below an indefinite series of horizontal straight lines.

The tracing-arms of those circles (each of which arms is the radius of the circular orbits of the particles contained in the trochoidal surface which it traces) diminish in geometrical progression with a uniform increase of the vertical depth at which the centre of the rolling circle is situated.

The preceding propositions agree with the existing theory, except that they are more comprehensive, being applicable to large as well as to small displacements.

The following proposition is new.

Proposition IV.—The centres of the orbits of the particles in a given surface of equal pressure stand at a higher level than the same particles do when the liquid is still, by a height which is a third proportional to the diameter of the rolling circle and the length of the tracing-arm (or radius of the orbits of the particles), and which is equal to the height due to the velocity of revolution of the particles.

Corollaries.—The mechanical energy of a wave is half actual and half potential—half being due to motion, and half to elevation.

The crests of the waves rise higher above the level of still water than their hollows fall below it; and the difference between the elevation of the crest and the depression of the

hollow is the double of the quantity mentioned in Proposition IV.*

The hydrostatic pressure at each individual particle during the wave-motion is the same as if the liquid were still.

Friction between a Wave and a Wave-shaped Solid.

In an Appendix to the paper is given the investigation of the problem, to find approximately the amount of the pressure required to overcome the friction between a trochoidal wave-surface and a wave-shaped solid in contact with it. The application of the result of this investigation to the resistance of ships was explained in a paper read to the British Association in 1861, and published in various engineering journals in October of that year. The following is the most convenient of the formulæ arrived at. Let w be the heaviness of the liquid; f the coefficient of friction; g gravity; v the velocity of advance of the solid; L its length, being that of a wave; z the breadth of surface of contact of the solid and liquid; β the greatest angle of obliquity of that surface to the direction of advance of the solid; P the force required to overcome the friction; then

$$P = \frac{fwv^2}{2g} Lz(1 + 4 \sin^2 \beta + \sin^4 \beta).$$

In ordinary cases, the value of f for water sliding over painted iron is .0036. The quantity $Lz(1 + 4 \sin^2 \beta + \sin^4 \beta)$ is what has been called the "augmented surface." In practice, $\sin^4 \beta$ may in general be neglected, as being so small as to be unimportant.

Glasgow, September 30, 1862.

LVIII. *On the Absorption and Radiation of Heat by Gaseous Matter.*—Second Memoir. By JOHN TYNDALL, F.R.S., Professor of Natural Philosophy in the Royal Institution.

[Concluded from p. 350.]

§ 12. SINCE these researches were commenced, an eminent experimenter has been led by his own inquiries in another field to enter upon the investigation of gaseous diathermancy. On the 7th of February of the present year (1861), Prof. Magnus communicated to the Academy of Sciences in Berlin a

* Mr. Stokes's investigation of the motion of waves in deep water, which is carried to the third approximation, leads to the same result with Prop. IV. as regards the difference between the elevation of the crests and the depression of the hollows. According to that investigation, however, the revolving motion of the particles is combined with a translation having a velocity that diminishes rapidly as the depth increases. No such translation is indicated by the investigation in the present paper.

memoir on the Transmission of Heat through Gases*. The published notices of my experiments, commencing in May 1859, had escaped his attention, and his work is therefore to be regarded as independent of mine. Considering the very different methods which we have pursued, the general agreement between us must be regarded as remarkable.

The starting-point of Professor Magnus's investigation was the interesting experiment of Mr. Grove, in which a platinum wire raised to whiteness by an electric current is suddenly cooled by an atmosphere of hydrogen. This action, which we have hitherto been disposed to attribute to the mobility of hydrogen, and its consequent high convective power, Professor Magnus was led to regard as an effect of conduction; and the thought induced him to examine the conductivity of gases generally. The mode of experiment adopted led him, not I think to the establishment of gaseous conductivity, but to results substantially the same as those that I had previously obtained. In fact the very experiments devised to show conductivity showed in a very striking manner the existence of athermancy, or opacity to radiant heat, in the case of a considerable number of gases.

The experiments on radiation, where obscure heat was made use of, were thus conducted. Two glass vessels, one much larger than the other, had their bottoms fused together; the larger one being turned upside down, the smaller one stood upright on the top of it. The mouth of the larger vessel was ground down, so that it could be placed like an ordinary receiver on the plate of an air-pump and exhausted, while through proper openings different gases could be afterwards admitted into it.

To the plate of the air-pump on which the above vessel was placed, was attached a thermo-electric pile with wires leading from it, through the plate, to a galvanometer; the axis of the pile was vertical, one face of it being turned downwards towards the plate, and the opposite face turned upwards towards the common surface of the two vessels which had been fused together.

Water was placed in the uppermost vessel, and caused to boil by conducting hot steam through it. Its under surface became thus heated to a temperature of 100° C. But this under surface constituted the upper surface of the vessel underneath. This latter, therefore, possessed a temperature of 100° C.; and it formed the source of heat made use of in the experiments.

Here Professor Magnus had a radiating surface of glass—a good radiator—kept at a constant temperature by the hot water above it; at a distance from this surface and turned towards it was the thermo-electric pile, defended from the radiation of the

* Poggendorff's *Annalen*, reprinted in *Philosophical Magazine*, S. 4. vol. xxii. p. 85.

surface, or exposed to it, at pleasure, by the action of a moveable screen. The entire space between the pile and the radiating surface could either be rendered a vacuum, offering no resistance to the passage of the calorific rays, or else be filled by a gas the diathermancy of which was to be examined.

The concurrence of the experiments made with this apparatus and those made with mine is, as I have stated, remarkable. Some differences, however, exist between my friend and myself, a few remarks on which will not be without their use to those who may afterwards enter upon this extensive field of inquiry.

Experimenting in the ordinary way with his thermo-electric pile—using one of its faces only—Professor Magnus finds that air and oxygen cut off each more than 11 per cent. of the heat emanating from his source, while hydrogen cuts off more than 14 per cent.* I, on the contrary, with the most delicate means I could apply, failed to establish the absorption of these gases by experiments made in the ordinary manner†. In fact it was their neutrality that drove me to devise the principle of compensation, briefly referred to at the commencement of this memoir. I was so particular in the experiments which led me to the above negative result, that if the absorption amounted to one-tenth of that found by Professor Magnus I do not think it could have escaped me. Nor do I think that if such an action existed Melloni could have concluded that the absorption of a column of air fifteen times the length of that employed by Professor Magnus was absolutely insensible.

In the account of the experiments already published, where my source of heat was also 100° C., I have set down the absorption of air, oxygen, and hydrogen at about 0·33 per cent., which is for air and oxygen thirty times, and for hydrogen over forty times less than that found by Professor Magnus.

In fixing the above figure for the absorption of these gases, I protected myself by assigning what I knew to be the superior limit of the effect, but I was morally certain at the time that as soon as I could combine sufficient power and delicacy I should make the effect less. This I have done in my present inquiry, and find the absorption of the above gases to be under 0·1 per cent., which in the case of oxygen is less than $\frac{1}{100}$ th, and in the case of hydrogen less than $\frac{1}{140}$ th of the effect obtained by Prof. Magnus with a tube less than half the length of mine. Making every allowance for the difference between our two sources of heat, the discrepancy between us is still enormous. In fact my conclusion is that these gases are practical vacua to radiant heat, and that the mixture of oxygen and nitrogen which constitutes the body of our atmosphere is the same.

* Page 30.

† Philosophical Transactions, 1861; and Phil. Mag. S. 4, vol. xxii. p. 169.

While, however, in the case of the elementary gases the discrepancy between Professor Magnus and myself consists in a defect on my part, or an excess on his, with the powerful gases I obtained a considerably stronger action than he does. Thus with olefiant gas his absorption amounts to less than 54 per cent., whereas in mine it amounts to more than 72. This last result is only what might be expected, inasmuch as the length of gas traversed by the radiant heat was in the one case a little under 15 inches, and in the other 33.

Professor Magnus has further published an account of experiments in which a powerful gas-flame surrounded by a glass cylinder furnished the source of heat, the latter being augmented by a parabolic mirror of polished metal placed behind the lamp. In this case the gases were enclosed in a glass tube 1 metre long and 35 millims. in diameter, the two ends of which were stopped with plates of glass 4 millimetres thick.

Two series of experiments were executed with this tube, in one of which the interior surface was covered with black paper, while in the other the glass was uncovered within. The former method is that pursued by Dr. Franz; and the result obtained by Professor Magnus in the case of atmospheric air and oxygen closely agrees with that obtained for the same gases by Dr. Franz. Professor Magnus makes the absorption in the case of the blackened tube about $2\frac{1}{2}$, and Dr. Franz about 3 per cent., for air and oxygen.

In the case of the unblackened tube, however, the absorption was found to be much more considerable. Here the absorption by air and oxygen amounted to 14.75 per cent., and with hydrogen it reached 16.23. This great difference between the unblackened and the blackened tube is ascribed by Professor Magnus to a change of quality which the heat undergoes by its reflexion from the interior glass surface.

One of my motives in introducing a glass tube into the present inquiry was, that I might be enabled to investigate the interesting question raised by this surmise of Professor Magnus. I have failed, however, to obtain his result. My naked glass tube, which is nearly of the same length as his, gives me a result which is more than 140 times less than his in the case of air and oxygen, and more than 160 times less than what he has obtained with hydrogen. Our sources of heat are, it is true, different, but the disadvantage is on my side; for assuredly the rays from a gas-jet are, if anything, less affected by the transparent elementary gases than those from my source. Had I time, I would repeat the experiments with a flame; but this, I regret to say, is out of my power at present.

Another difference between Professor Magnus and myself has
Phil. Mag. S. 4. Vol. 24. No. 163. Dec. 1862.

reference to the influence of aqueous vapour. With both the gas-flame and the boiling water as sources of heat, he finds the effect of dry air to be precisely the same as that of air which he has allowed to pass in minute bubbles through water, and thus saturated with aqueous vapour.

I was engaged in experiments on this substance when my other duties compelled me to close this inquiry for a time. I believe, however, I may safely say that not only is the action of aqueous vapour on radiant heat measurable, but *this action may be made use of as a measure of atmospheric moisture, the tube used in my experiments being thus converted into a hygrometer of surpassing delicacy.* Unhappily, as in other cases touched upon in this memoir, I have been unable to give this subject the development I could wish; but the results which I am in a position to record are nevertheless interesting,

On a great number of occasions I compared the air sent in directly from the laboratory into the experimental tube with the same air after it had been passed through the drying-apparatus. Calling the action of the dry air unity, or supposing it rather to oscillate about unity (for the temperature of my source varied a little from day to day), on the following days the annexed absorptions were observed with the undried air of the laboratory:—

Absorptions by undried air.

October 23rd . . . 63	November 1st . . . 50
October 24th . . . 62	November 4th . . . 58
October 29th . . . 65	November 8th . . . 49
October 31st . . . 56	November 12th . . . 62

Nearly $\frac{9}{10}$ ths of the above effects are due to aqueous vapour; which, therefore, in some instances *exerted nearly sixty times the action of the air in which it was diffused.*

The experiments which I have made on aqueous vapour have been very numerous and varied. Differing as I did from so cautious and able an experimenter, I deemed it due to Professor Magnus and myself to spare no pains in securing myself against error. I have experimented with air moistened in various ways, sometimes by allowing small bubbles of it to ascend through water, sometimes dividing it by sending it through the pores of common cane immersed in water. Between the drying-apparatus and the experimental tube I have introduced tubes containing fragments of glass moistened with water, and allowed the air to pass over them; large effects were in all such cases obtained, the absorption being usually *more than eighty times that of dried air.* Fragments of unwetted glass, which had been merely exposed to the air of the laboratory, had dry air led over

them into the experimental tube; the absorption was fifteen times that of dried air. A roll of bibulous paper, taken from one of the drawers of the laboratory, and to all appearance perfectly dry, was enclosed in a glass tube, and dry air carried between its leaves. The experiment was made five times in succession with the same paper, and the following absorptions were observed:—

	Absorption.
No. 1	72
No. 2	62
No. 3	62
No. 4	47
No. 5	47

In fact, the action of aqueous vapour is exactly such as might be expected from the vapour of a liquid which Melloni found to be the most powerful absorber of radiant heat of all he had examined.

Every morning, on commencing my experiments, I had an interesting example of the power of glass to gather a film of aqueous vapour on its surface. Suppose the tube mounted, and the air of the laboratory removed, as far as the air-pump was capable of removing it. On allowing dry air to enter for the first time, the needle would move from 0° to 50° . On pumping out it would return to 0° , and on letting in dry air a second time it would swing almost to 40° . Repeated exhaustions would cause this action to sink almost to nothing. These results were entirely due to the vapour collected during the night in an invisible film on the inner surface of the tube, and which was removed by the air on entering, and diffused through the tube. If the dry air entered at the end of the tube nearest to the source of heat, on the first and second admissions, and sometimes even on a third, the vapour carried from the warm end to the cold end of the tube was precipitated as a mist upon the latter, for a distance sometimes of nearly a foot. The mist always disappeared on pumping out. It is needless to remark that facts of this character, of which I could cite many, were not calculated to promote incautiousness or rashness on my part. I saw very clearly how easy it was to fall into the gravest errors, and I took due precautions to prevent myself from doing so.

Knowing that a solution of salt was almost as opaque to radiant heat as water itself, I was careful to examine whether the effects which I had observed with aqueous vapour might not be due to the precipitation of the vapour on the surfaces of the plates of salt used to stop my tube. The substance is well known to be very hygroscopic; and during the last three years

the knowledge of this fact has rendered me careful to remove my polished plates every evening from the apparatus, and to keep them in perfectly dry air. Still, when it is remembered that the air on entering the tube is raised in temperature and thus enabled to maintain a greater amount of vapour, and that the tube and plates of rock-salt form the channel for a flux of heat from the radiating source, the likelihood of precipitation occurring will seem but small. On examining the plates after the undried air of the laboratory was experimented with, no trace of precipitated moisture was observed upon their surfaces.

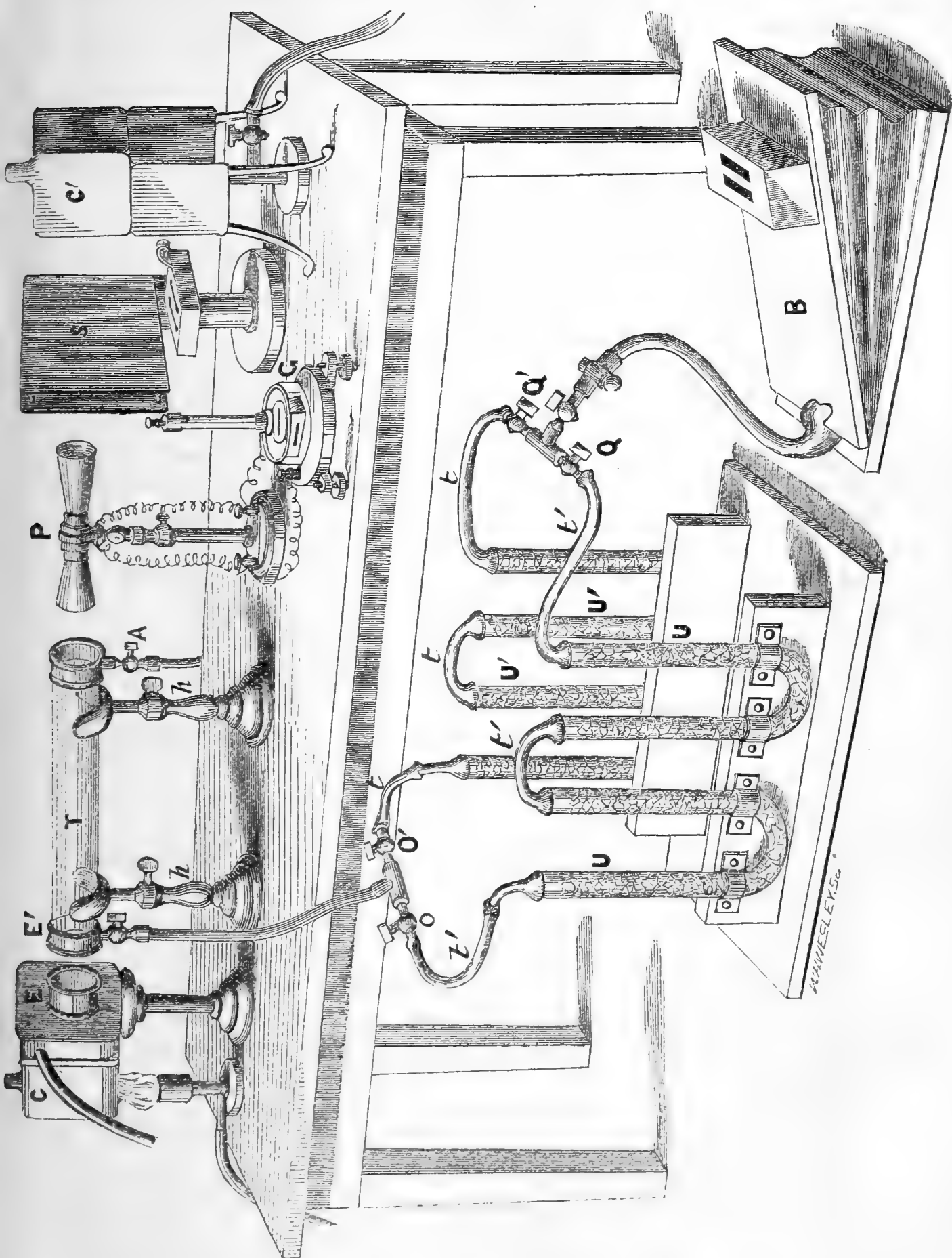
But, to place the matter beyond all doubt, I abolished the plates of rock-salt altogether, and operated thus:—An india-rubber bag (B) was filled with air, and to its nozzle a T-piece, with the cocks Q Q', was attached. The cock Q' was connected with two tubes, U' U', each of which was filled with fragments of glass moistened with distilled water. The cock Q was connected with the tubes U U, each of which was filled with fragments of glass moistened by sulphuric acid. The other ends of these two series of tubes were connected with the cocks O O'; and from the T-piece between these cocks a tube led to the end E' of the open experimental tube T. The cock A at the other end of the experimental tube was placed in connexion with an air-pump. The pile P, the screen S, and the compensating cube C' were used as in the other experiments. E is the end of the front chamber, and C the source of heat. In some experiments I had the end E closed by a plate of rock-salt, in others it was allowed to remain open, a distance of about 12 inches intervening between the radiating surface and the open end E' of the experimental tube.

Closing the cocks Q and O, and opening Q' and O', gentle pressure being applied to the bag B, a current of moist air was slowly discharged at the end E' of the experimental tube. The pump in connexion with A was then worked; and thus by degrees the air was sucked into the tube T. The deflection of the galvanometer was 30°, when the moist air filled the tube as completely as the arrangement permitted*,—this deflection being due to the predominance of the compensating cube over the radiating source C.

The cocks Q' and O' were now closed, and Q and O opened; proceeding as before, a current of *dry* air was discharged at E', and this air was drawn into the tube T in the manner just described. The moist air was thus displaced by dry; and, while the displacement was going on, the galvanometer was observed through the distant telescope. The needle soon commenced to sink, and slowly went down to zero, proving that a greater quan-

* Still, of course, only partially.

tity of heat passed through the dry than through the moist air. The wet air was substituted for the dry, and the dry for the wet



twenty times in succession, with the same constant result: the entrance of the humid air caused the needle to move from 0° to

30°, while the entrance of dry air caused it to fall from 30° to 0°. The air-pump was resorted to, because I found that when I attempted to displace the air by the direct force of the current from B, the temperature of the pile, or of the source, was so affected by the fresh air as to confuse the result. I may remark that not only have I operated thus for days with aqueous vapour, but every result which I have obtained with vapours generally has been thus confirmed, so that all doubt as to the applicability of the rock-salt plates to researches of this nature may, I think, be abandoned*.

§ 13. Whence, then, arise those differences between Professor Magnus and myself? I have no doubt that every one of his published results is the record of an experiment made with the utmost care which it is possible to bestow upon scientific work. The differences between us are, I imagine, to be referred to a radical defect in his apparatus. His desire was to do away with plates of all kinds between his source of heat and his pile, and hence he brought his gas *into direct contact with his source of heat*. The same thought had occurred to myself, and I was on the point of falling into the same error; but a series of experiments executed with reference to this point, so early as the 26th of July 1859, showed me that the accuracy of the results was entirely compromised by bringing the gas to be examined into contact with the source. I obtained thus an action forty times what I knew it ought to be, and was confirmed in the view which caused me to interpose a vacuous chamber in front of the experimental tube. Let me here record a few experiments made on the 4th of last November in connexion with this subject.

I first satisfied myself that the drying-apparatus was in perfect condition, the air of the laboratory producing, when sent through it, an absorption of 1. This same air was sent into the front chamber, that is, into direct contact with the source. The galvanometer needle moved as it does in the case of absorbent gases, and at the end of two minutes declared a loss of heat equivalent to an absorption of 50. The front chamber is 8 inches in length; the experimental tube is 33 inches long; hence a column of 8 inches, in contact with the radiating surface, produced at least fifty times the effect of a column more than four times as long when the air was separated from the radiating surface.

I made the foregoing experiment three times in succession, and after two minutes found the needle pointing to precisely the same degree; the lowering of the source was perfectly constant and regular, and in all cases showed a loss equivalent to an absorption of 50.

* It is sheer want of time that prevents me from describing more particularly the numerous experiments executed with open tubes.

It will be remembered that Professor Magnus obtained a greater absorption with hydrogen than with either oxygen or air. This result is perfectly explained by reference to the quicker convection of this gas. I operated with hydrogen as I did with air, first satisfying myself that a column of it 33 inches long exercised an absorption less than unity: in fact it could not be measured. The same hydrogen introduced into the first chamber, and allowed to remain there for two minutes, caused a withdrawal of heat from the source equivalent to an absorption of 65. Now the absorption of air in Professor Magnus's experiments is to that of hydrogen as

$$11.12 : 14.21,$$

or as

$$50 : 64,$$

while my results of convection are as

$$50 : 65.$$

The coincidence is so perfect that I am disposed to regard it as in part accidental.

Substantially the same remarks apply to the experiments with the glass tube stopped with plates of glass 4 millimetres thick. According to Melloni, 61 per cent. of the rays of a Locatelli lamp are absorbed by a plate of glass only 2.6 millimetres thick. Professor Magnus surrounded his flame by a glass cylinder; and this, it may be urged, partially sifted the heat of the lamp before it reached the end of the tube. But in so doing the glass cylinder itself must become intensely heated; and to the heat of the cylinder the glass ends of the tube would be *opaque*; they would absorb it all. Cold air admitted into such a tube is exactly similar to cold air let into my front chamber; it chills what is in part the source of heat, and maintains that chill by convection. The heat applied may, in fact, be thus analysed. 1. We have a portion, almost wholly luminous, which went through the tube direct to the pile; 2, a portion *arrested* by the first glass plate; 3, a smaller portion *arrested* by the second glass plate; 4, we have the heat *radiated* by the first glass plate towards the second, and wholly absorbed by the latter; 5, we have the heat radiated by this latter against the pile. This analysis will probably enable us to understand how Professor Magnus obtained an absorption of only $2\frac{1}{2}$ per cent. with the blackened tube, and as much as 14.75 per cent. with the unblackened one. With the latter, the source and the plate of glass nearest the source send a copious flux down the tube to the plate at the opposite end; the oblique rays are in great part reflected by the interior surface, and thus reach the plate adjacent to the pile. With the blackened tube this oblique radiation is entirely cut off, the rays

incident on the interior surface being absorbed. Thus the plate of glass adjacent to the pile must be much more intensely heated with the unblackened tube than with the blackened one. The difference in the amount of heat received by the pile-end plate in the respective cases is rendered very manifest by the experiments of Professor Magnus himself; for he finds that, with the same source, twenty-six times the amount of heat transmitted by the coated tube is transmitted by the uncoated one. What, therefore, Professor Magnus ascribes to a change of quality by reflexion, would, if I am correct, be due to the higher heating in the case of the naked tube, and consequent *greater chilling by the cold air*, of the plate of glass close to the pile. To this must be added the effect produced by cooling the distant end of the tube itself, to which heat has been communicated from the first glass plate by the process of conduction, and the cooling of which comes most into play when the tube is uncovered.

The difference between Professor Magnus and myself as regards the action of aqueous vapour admits now of easy explanation. His effect being one of convection, and not of absorption, the quantity of vapour present in his experiments—probably not more than 1 per cent. of the volume of the gas, certainly not 2 per cent.—vanished as a convecting agent, in comparison with the air.

It is hardly necessary to repeat these reflections with reference to the experiments of Dr. Franz. The taking of the chilling of his plates for absorption has caused him to find no difference of effect when he doubled the length of his tube. With a tube 450 millimetres long, he finds precisely the same absorption as with a tube of 900. He finds the action of carbonic acid to be the same as that of air, although at atmospheric tensions the action of the former is 90 times that of the latter*. He finds the vapour of bromine more destructive to radiant heat than nitrous acid gas, whereas the latter is beyond comparison the most destructive. The heat rendered latent by the evaporation of the bromine of course augmented the chill, and thus magnified the effect which in reality he was measuring. In reference to heating the glass plates by the flame made use of in his experiments, I will cite a single passage from the memoir of Dr. Franz. It refers to the vapour of iodine produced by throwing the substance on a heated surface in a vessel closed with glass plates. "The mirror," he writes, "showed a deflection of only 178.

* The sensible equality of all the transparent gases and air was regarded as evident by Dr. Franz. "It might be seen," he writes, "from the outset that no decided difference would be observed between them" (p. 342). Similarly, Professor Magnus, speaking of aqueous vapour, writes, "Although it might be foreseen with certainty that the small amount of aqueous vapour in the air could have no influence on the radiation," &c. (p. 43).

But as the glass plates through which the heat radiated had not yet assumed a temperature high enough to reduce the iodine, which had been precipitated upon them in crystals, to a state of vapour it was necessary to wait, and allow the radiation of the lamp to continue till all the iodine was driven from the bottle”*. This shows how much the glass plates could be heated by the radiation of the lamp, this heat on a particular occasion being sufficient to dissipate the solid iodine which had coated the glass plates.

§ 14. As a dam built across a river causes a local deepening of the stream, so our atmosphere, thrown as a dam across the terrestrial rays, produces a local heightening of the temperature at the earth’s surface. This, of course, does not imply indefinite accumulation, any more than the river dam does, the quantity lost by terrestrial radiation being, finally, equal to the quantity received from the sun. The chief intercepting substance is the aqueous vapour of the atmosphere†, the oxygen and nitrogen of which the great mass of the atmosphere is composed being sensibly transparent to the calorific rays. Were the atmosphere cleansed of its vapour, the temperature of space would be directly open to us; and could we under present circumstances reach an elevation where the amount of that vapour is insensible, we might determine the temperature of space by direct experiment. Colonel Strachey has written an admirable paper on the aqueous vapour of the atmosphere‡, in which he shews that the amount of vapour diminishes much more speedily with the elevation than might be inferred from the law of Dalton.

It might be possible to reach a height where, by preserving one face of a thermo-electric pile at the temperature of the locality, the other, protected from all terrestrial radiation, turned to the zenith, would assume the temperature of space in that direction§, while the consequent galvanometric deflection would give us the means of determining the difference in temperature between the two faces of the pile. Knowing one, we should therefore be able to determine the other; knowing the temperature of the locality, we could infer from it the temperature of stellar space.

* “Es musste bei fortdauernder Strahlung der Lampe den Zeitpunkt abgewartet werden.”

† The mildness of an island climate must be in part due to this cause. The direct tendency of the vapour is to check sudden fluctuations of temperature. Where it is absent, as at the surface of the moon, such fluctuations must be enormous. The face turned towards the sun drinks in the solar rays without let or hindrance, while the radiation of the face turned from the sun pours unchecked into space.

‡ Phil. Mag. S. 4. vol. xxiii. p. 152.

§ A well of cold air would be formed within the reflector, the lowest stratum of the well sharing the temperature of the face of the pile.

Many eminent writers, it is true, have supposed the upper atmospheric regions to be colder than space, the depression of temperature being due to the radiation of the aërial particles, just as a grass-blade is lowered, by its radiation, below the air which surrounds it. This notion must, I think, be abandoned; for, as far as experiment goes, it leads us to conclude that air, and particularly air in the higher atmospheric regions, behaves as a vacuum both as regards radiation and absorption.

§ 15. In his paper on the conduction of heat by gases, Professor Magnus examines the question of convection, and has adduced some striking experiments to show that the cooling of an incandescent wire in hydrogen is not due to the convection of the gas. He finds that when the wire is enclosed in a narrow tube, with only a thin film of the gas surrounding it, and where therefore currents, in the ordinary sense, are hard to be conceived of, the gas still exercises its cooling power. It had often occurred to me to make this experiment; and when I first heard of its successful performance by Professor Magnus I adopted his conclusion, that the cooling was due to conduction.

Reflection, however, caused me to change my opinion. Suppose the wire to be stretched along the axis of a wide cylinder containing hydrogen, we should have convection, in the ordinary sense, on heating the wire. Where does the heat thus dispersed ultimately go? It is manifestly given up to the sides of the cylinder. The transfer by convection is a transfer ultimately to the sides of the cylinder, and if we narrow our cylinder we simply hasten the transfer. The process of narrowing may continue till a tube like that used by Professor Magnus is the result; the convection between centre and sides will still continue, and produce the same cooling effect as before. Whether we assume conduction or convection, the tube surrounding the wire must be supposed to possess sufficient conducting power to carry the heat off, otherwise it would become incandescent itself by the accumulation of the heat.

The reasoning of Professor Magnus in connexion with this subject is of extreme ingenuity. He contends that there is no reason why stronger currents should establish themselves in hydrogen than in other gases. Currents are due to differences of density produced by the expansion of a portion of the gas by heat. Now hydrogen actually expands *less* than other gases, and hence the differential action on which the currents depend is less in this gas than in the others. Professor Magnus alludes to the friction of the particles against each other, but considers this ineffective.

This reasoning leads us to the threshold of a question which might form the subject of a long and profitable investigation.

For a given difference of density, is not the mobility of hydrogen greater than that of the other gases? The experiments above recorded, where different gases were brought into direct contact with the source of heat, seem to answer this question in the affirmative. I have had no time to pursue the question regarding hydrogen; but I have made a few experiments which show the influence of density on the mobility of a gas in a very striking manner.

Having first so purified atmospheric air as to render it sensibly neutral to radiant heat, I allowed 15 inches of it to enter the front chamber F, and there to come into contact with the source of heat. Convection of course immediately set in, and its amount was accurately measured by the quantity of heat withdrawn from the radiating surface; this quantity, expressed in the units adopted throughout this memoir, was 62.

The quantity of gas in the front chamber was now doubled, that is, it now had an atmosphere of tension; the withdrawal of heat then was expressed by the number 68.

In the last experiment we had double the number of atoms loading themselves with heat and carrying it away; if their motion had been as quick as that of the atoms when half an atmosphere was used, they would have withdrawn sensibly double the amount of heat; but the fact is that half an atmosphere carried off 62, while a whole atmosphere carried off 68; hence the absolute swiftness of the atoms in the case of the denser air must be very much less than in the case of the rarer. In fact, the amount of heat withdrawn will be proportional on the one hand to the number of carrying particles, and on the other to the velocity with which they move; hence if v and v' be these velocities, we have

$$\frac{62}{68} = \frac{v}{2v'}, \text{ or } \frac{v}{v'} = \frac{62}{34}.$$

Thus, while the atoms of the rarer gas travel 62 units in a second, those of the denser gas travel only 34.

This retardation can, I think, arise from nothing else than the resistance offered by the particles of the air to the motion of their fellows. It must be borne in mind that the smallness of the increment observed on doubling the amount of gas was not due to the partial exhaustion of the source by the first quantity of gas. The heat of the source was such that the withdrawal of 64 of our units could not sensibly affect the subsequent convection.

Here, then, we see what a powerful effect density, or the internal resistance which accompanies density, has on the mobility of a gas; and there is every reason to suppose that the mobility of hydrogen is due to the comparative absence, in its

case, of internal resistance. However this may be, the foregoing experiment enables us to draw some important inferences.

Storms at great heights must be greatly facilitated by the mobility of the particles of the air. In fact storms are cases of convection on a large scale, and in our front chamber we had one in miniature. With the same difference of temperature on the summit of Mont Blanc, the motion of convection would be very nearly twice as great as at the sea-level.

In the summer of 1859 I was fortunate enough to induce my friend Professor Frankland to accompany me to the summit of Mont Blanc, and to determine the comparative rates of combustion there and in the valley of Chamouni. Six candles were purchased, burnt for an hour at Chamouni, and the loss of weight determined. The same candles were lighted for the same time on the summit of the mountain, and the consumption determined. Within the limits of error, the consumption above was equal to that below. The *light* below was immensely greater than that above, still the amount of stearine consumed in the two cases was sensibly the same. Professor Frankland surmised this to be due to the greater mobility of the rarefied air, which allowed a freer interpenetration of the flame by the oxygen*; and the foregoing experiments show that the augmentation of mobility is just such as would account for the observed effect.

LIX. On the Integral of the general Equation in Differences.

By J. J. SYLVESTER, A.M., F.R.S.†

THE most general form which can be given to a linear equation in differences may easily be seen to be reducible to the following,

$$a_x u_x + b_x u_{x-1} + c_x \cdot u_{x-2} + \&c. \text{ ad lib. } = 0,$$

with the initial conditions

$$u_0 = 1, \quad u_{-e} = 0.$$

Consequently to find u_n , or let us rather say to find

$$(-)^n a_1 a_2 \dots a_n u_n,$$

is really the problem of finding the value of a determinant belonging to a matrix of n^2 terms, whereof all the places below the

* The influence of interpenetration is well seen in the exposed gas-jets of London, particularly in the butcher's shops on a Saturday night. A gust of wind, which carries oxygen to the centre of a flame, suddenly deprives it of light. A simple and beautiful experiment consists of passing a lighted candle swiftly to and fro through the air; the white light reduces itself to a pale-blue band. Bunsen's burner is an illustration in the same line.

† Communicated by the Author, having been read at the Meeting of the British Association at Cambridge, October 1862.

diagonal line, with the exception of those in the oblique line immediately under the diagonal, are occupied by zeros, but of which all the other places are or may be occupied by finite quantities. For instance, supposing n to be 4, such a determinant would be

$$\begin{matrix} b_4 & c_4 & d_4 & e_4 \\ a_3 & b_3 & c_3 & d_3 \\ 0 & a_2 & b_2 & c_2 \\ 0 & 0 & a_1 & b_1 \end{matrix}$$

Let us for a moment consider more particularly this determinant. If, using double indices to denote each coefficient, we were to write the above according to the usual method of notation as below,

$$\begin{matrix} 4.4 & 4.3 & 4.2 & 4.1 \\ 3.4 & 3.3 & 3.2 & 3.1 \\ 0 & 2.3 & 2.2 & 2.1 \\ 0 & 0 & 1.2 & 1.1 \end{matrix}$$

the law of formation of the general term would be very far from becoming evident on a cursory inspection; but a slight change, suggested by the very system of equations in which the determinant originates, makes the law at once obvious. Nothing is more natural than that we should use $r.s$ or $s.r$, where $r > s$, to denote the coefficient of u_s in the equation of which r is the highest subindex of u ; with this modification, the above determinant changes into the following:—

$$\begin{matrix} 4.3 & 4.2 & 4.1 & 4.0 \\ 3.3 & 3.2 & 3.1 & 3.0 \\ . & 2.2 & 2.1 & 2.0 \\ . & . & 1.1 & 1.0 \end{matrix}$$

(the terms with equal indices appearing not now in the diagonal, but in the oblique line below it). With this notation it becomes apparent (and the *reason* of the rule may be deduced by the most simple reasoning from following the course of the successive substitutions in the system of equations giving rise to the determinant) that to find the general term we must write all the descending series of integers which can be formed, beginning with 4 and ending with zero, viz.

- 43210
- 4310 .
- 4210
- 4320
- 430
- 420
- 410
- 40

and read them off respectively into products as below :—

$$\begin{aligned}
 &4.3 \times 3.2 \times 2.1 \times 1.0 \\
 &(4.3 \times 3.1 \times 1.0) \times (-2.2) \\
 &(4.2 \times 2.1 \times 1.0) \times (-3.3) \\
 &(4.3 \times 3.2 \times 2.0) \times (-1.1) \\
 &(4.3 \times 3.0) \times (2.2 \times 1.1) \\
 &(4.2 \times 2.0) \times (3.3 \times 1.1) \\
 &(4.1 \times 1.0) \times (2.2 \times 3.3) \\
 &(4.0) \times (1.1 \times 2.2 \times 3.3)
 \end{aligned}$$

The sum of the above terms is the value of the determinant in question. And so in general, if we define u_n by means of the equation

$$(n \cdot n)u_n + (n \cdot n-1)u_{n-1} + (n \cdot n-2)u_{n-2} + \dots = 0;$$

with the initial conditions as above stated, the value of u_n to a factor *près* will be represented by

$$\Sigma(n, n_1, n_2, \dots, n_\omega, 0),$$

where $n > n_1 > n_2 > \dots > n_\omega$ [$\omega = 0, 1, 2, \dots, (n-1)$] and $(n, n_1, n_2, \dots, n_\omega, 0)$ is to be interpreted as meaning

$$M \times n \cdot n_1 \times n_1 \cdot n_2 \times \dots \times n_\omega \cdot 0,$$

where to find M we write the complementary integers

$$m_1, m_2, m_3, \dots, m_{n-\omega+1},$$

which together with $n_1, n_2, \dots, n_\omega$ make up the complete tally of all the integers from 1 to $(n-1)$, and then write

$$M = (-)^{n-\omega+1} (m_1 \cdot m_1) \cdot (m_2 \cdot m_2) \dots (m_{n-\omega+1} \cdot m_{n-\omega+1}).$$

In order to form by an exhaustive process all the descending series above described, we may if we please consider the differences of the terms of any such series, and write

$$\delta = n - n_1, \quad \delta_1 = n_1 - n_2 \dots \delta_\omega = n_\omega,$$

we have then

$$\delta + \delta_1 + \delta_2 + \dots + \delta_\omega = n.$$

So that the question is reducible to that of finding all the partitions of n , and of permuting in every possible manner the terms in each such system of partitions; for it is obvious that in general the value of $(n, n_1, n_2, \dots, n_\omega, 0)$ depends not only on the magnitudes, but on the order of sequence of $\delta, \delta_1, \delta_2, \dots, \delta_\omega$.

If we suppose that the order of the differences is limited, as, for example, that the equation is of the i th order, then any such coefficient as $r \cdot s$ is to be considered as zero when $r \cup s > i$, and consequently the partitions of n are to be limited to parts none greater than i . Moreover, if in such case the coefficients become constant, so that $r \cdot s = \phi(r-s)$, it is apparent that the order of

the arrangement of $\delta_1, \delta_2, \dots \delta_\omega$ becomes indifferent, and consequently the value of u_n , defined by the equation

$$u_n = (1)u_{n-1} + (2)u_{n-2} + \dots + (i)u_{n-i},$$

becomes the coefficient of t^n in $\frac{1}{1 - (1)t - (2)t^2 - \dots - (i)t^i}$, as is well known.

The above rule may easily be extended to a linear equation in differences with any number of variables. Thus suppose, for greater simplicity, that we write

$$u_{x,y} = \Sigma \binom{x, x'}{y, y'} u_{x', y'} \left[\begin{matrix} x' = x-1, x-2, \dots 0 \\ y' = y-1, y-2, \dots 0 \end{matrix} \right],$$

with the initial conditions $u_{0,0} = 1, u_{e,f} = 0$ wherever one or both of e, f are negative units; then to find the value of m, n we must form all the possible descending series $\left[\begin{matrix} m, m_1, m_2, \dots m_\omega, 0 \\ n, n_1, n_2, \dots n_\omega, 0 \end{matrix} \right]$, subject only to the law that there is a descent either from m_i to m_{i+1} , or from n_i to n_{i+1} , or at one and the same time from m_i to m_{i+1} and from n_i to n_{i+1} . The value of $u_{m,n}$ then becomes

$$\Sigma \binom{m, m_1, m_2, \dots m_\omega, 0}{n, n_1, n_2, \dots n_\omega, 0},$$

with the understanding that the term within the parenthesis is to be read as meaning

$$\binom{m, m_1}{n, n_1} \times \binom{m_1, m_2}{n_1, n_2} \times \binom{m_2, m_3}{n_2, n_3} \dots \times \binom{m_\omega, 0}{n_\omega, 0}.$$

And in like manner and under a similar form we obtain the value of $u_{n_1, n_2, \dots n_\epsilon}$ defined by the general equation

$$u_{n_1, n_2, \dots n_\epsilon} = \Sigma \binom{n_1, \nu_1}{n_2, \nu_2} \binom{n_2, \nu_2}{\vdots \vdots} \binom{\vdots \vdots}{n_\epsilon, \nu_\epsilon} u_{\nu_1, \nu_2, \dots \nu_\epsilon}.$$

In defining the relations which connect one u with another, we may suppose that (r, s) means the coefficient of u_s in the equation

$$u_r = \Sigma (r, s) u_s [r > s, u_0 = 1, u_{-e} = 0];$$

but we may also suppose that r, s means the coefficient of v_r in the equation

$$v_s = \Sigma (r, s) v_r [r > s, v_n = 1, v_{n+\epsilon} = 0];$$

the value of u_0 , on the latter supposition, it is obvious, becomes equal to that of u_n on the former—a fact that is well known, and

deducible from the circumstance that u_n and v_0 will be represented by the same determinant turned round into a new position. But by means of our general representation for the case of any number ϵ of variables, we see that there is an analogous theorem which connects together 2^ϵ different results, and which is not so immediate a consequence of the theory of determinants.

To make my meaning more clear, if we suppose the four following systems of equations, in each of which $m > \mu$, $n > \nu$,

$$u_{m,n} = \Sigma \binom{m, \mu}{n, \nu} u_{\mu, \nu} [u_{0,0} = 1, u_{-e,f} = 0, u_{e,-f} = 0, u_{-e,-f} = 0]^*,$$

$$v_{\mu,n} = \Sigma \binom{m, \mu}{n, \nu} v_{m,\nu} [v_{m,0} = 1, v_{m+e,0} = 0, v_{m-e,-f} = 0, v_{m+e,-f} = 0],$$

$$w_{m,\nu} = \Sigma \binom{m, \mu}{n, \nu} w_{\mu,n} [w_{0,n} = 1, w_{0,n+f} = 0, w_{-e,n-f} = 0, w_{-e,n+f} = 0],$$

$$\omega_{\mu,\nu} = \Sigma \binom{m, \mu}{n, \nu} \omega_{m,n} [\omega_{m,n} = 1, \omega_{m+e,n-f} = 0, \omega_{m-e,n+f} = 0, \omega_{m+e,n+f} = 0],$$

we shall have $u_{m,n} = v_{0,n} = w_{m,0} = \omega_{0,0}$.

The theorem $u_n = v_0$ above given, when the equation of differences is of the second order, expresses the well-known theorem that the cumulant $[a, b, c, \dots, h, k, l]$ (the denominator of the contained fraction $\frac{1}{a+}, \frac{1}{b+}, \frac{1}{c+}, \dots, \frac{1}{k+}, \frac{1}{l}$) is the same as the cumulant $[l, k, h, \dots, c, b, a]$.

There is no known property either of cumulants of this kind or those of the higher orders, nor can there be any found, but what does and must flow as an immediate consequence from the representation of the linear-difference integral above given. For instance, the law of formation of the above cumulant by rejecting consecutive pairs of terms becomes intuitive; for to meet this case we must write descending series of integers $n, n_1, n_2, \dots, n_\omega, 0$, such that each difference between consecutive terms n_i, n_{i+1} is always 1 or 2, and when the latter $(n_i, n_{i+1}) = 1$.

So more generally if we write $u_n = a_n \cdot u_{n-1} + u_{n-r}$, we obtain an analogous law for throwing out in every possible way groups of r consecutive terms in order to express u_n in terms of $a_n, a_{n-1}, a_{n-2}, \dots, a_0$. So, too, if we write $u_n = u_{n-1} + b_n \cdot u_{n-\mu}$, we obtain Binet's law of "*discontiguous*" products given in his long memoir on the subject published in the *Mémoires* of the Institute,—

* Or, more simply and rather more accurately, in place of the three equations within the bracket it is better to write $u_{p,q} = 0$ when p or q or each of them is negative, and so analogously for the cases following:—

$$v_{p,q} = 0 \text{ when } m-p \text{ or } q \text{ or each of them is negative,}$$

$$w_{p,q} = 0 \text{ when } m \text{ or } n-q \text{ or each of them is negative,}$$

$$\omega_{p,q} = 0 \text{ when } m-p \text{ or } n-q \text{ or each of them is negative.}$$

the law of descent upon this supposition being that the difference between n_i and n_{i+1} is 1 or r ; and if the former, $n_i, n_{i+1} = 1$.

We have seen above the convenience of shifting the system of subindices so as, for instance, to be able to treat the question of finding u_0 when we suppose $u_n = 1$ and $u_{n+e} = 0$, as well as that of finding u_n when we suppose $u_0 = 1$, $u_{-e} = 0$. More generally there is an advantage in writing $u_m = 1$ and $u_{m-e} = 0$ when it is a question of expressing u_n , which may then be conveniently denoted indifferently by $m : n$ or $n : m$,—the law being that regularly descending or ascending series are to be formed beginning with n and ending with m in every possible manner, each of which expresses a known product consisting of two parts—one made up of factors denoted by the conjunction of the consecutive terms in every such series, the other by the duplication of the integers between n and m not appearing in the series.

It is, moreover, convenient in some cases to express the limit which the descents are not to exceed (corresponding to the order of the equation). Thus $\frac{n : m}{i}$ may be used to denote the limita-

tion of the differences in $n : m$ not to exceed i . The well-known theorem in continued fractions ordinarily denoted by the equation $pq' - p'q = \pm 1$ may then be expressed in a somewhat more general form in the manner following.

[To be continued.]

LX. On the Action of various Coloured Bodies on the Spectrum.

By Sir DAVID BREWSTER, K.H., D.C.L., F.R.S. &c.*

IN various papers on light published in the Transactions of the Royal Societies of London and Edinburgh†, I have had occasion to mention the general results of a large number of experiments made on the action of numerous solids, fluids, and gases upon the solar spectrum; but the observations themselves, as recorded at the time when they were made, I have not found leisure to publish.

In order to understand these observations, and apply them in the study of the spectrum, it is desirable to have a copy of Fraunhofer's spectrum; but as many persons may not possess one, I have endeavoured to supply the want of it by the following nomenclature of the 354 lines observed by Fraunhofer, which in many respects will be found more useful than the spectrum itself.

N.B. When lines occur in groups, the numbers indicating them are generally placed in the same line.

* Communicated by the Author.

† Edinb. Trans. 1822, vol. ix. p. 433. Ibid. 1831, vol. xii. pp. 123, 519, 538. Phil. Trans. 1835, p. 91. Report Brit. Assoc. 1855, Trans. Sect. p. 7. Life of Newton, 2nd ed. vol. i. p. 103.

[illegible]

1. *Alkanet root*, an alcoholic solution, dark orange-red. At great thicknesses it absorbs all the rays more refrangible than D, that is, the part D H of the spectrum, eating out or absorbing *m* and *n*, and leaving C *m* a bright band.

In a diluted state it begins by widening *d* or D 2, E, *b*, and F, and also a line halfway between E and G. At a certain thickness the absorbed bands E and *b* nearly join, leaving a sharp luminous line between them. The space E F is then dark before the space D 2, E, and the blue and violet spaces have disappeared. *The blue, however, has become nearly violet.* At a greater thickness D 2 E disappears, and about the same time the *blue* space is almost gone, the *violet* remaining. At a greater thickness the *violet* disappears, and there remains only the bright band C *m*, the space D *d* gradually diminishing.

When some drops of the alcoholic solution are put into water so as to produce a fine deep lilac, the fluid acts still more powerfully upon the lines *m* and *n*, finally absorbing at an increased thickness the space between them. The bands *d* E and E F, which with B *m*, *n* *d* make four bands pretty equal in size, vanish almost simultaneously, the blue and violet still remaining, and the red and violet appearing to be persistent.

2. *Cochineal*, alcoholic solution, dark orange-red, but paler than No. 1. At great thicknesses it absorbs all the rays more refrangible than D, exhibiting *m* and C *m* slightly bright, and *n* very faintly.

When diluted, it first attacks δ and the whole space between D 5 and E, the space between δ and D 5 being simultaneously absorbed, leaving for an instant a brightish line on each side of F. *The blue then disappears after becoming slightly violet*, then the violet, the spectrum ending at D 4. In the red space *m* and *n* are both seen. In a strong light the violet will continue longer.

When the alcoholic solution is mixed with water so as to produce a fine lilac, it absorbs at great thicknesses all the rays more refrangible than D. It begins at *b* and D 2, and absorbs D F almost at the same time. *The blue becomes violet*, and then both disappear. An aqueous solution of cochineal is of a nut-brown colour, and acts pretty equally on all the rays of the spectrum.

3. *Chica*, alcoholic solution, dark orange-red like cochineal. At great thicknesses it absorbs all rays more refrangible than D 2, the violet disappearing last.

Diluted with alcohol it begins at F and D 5, absorbing rapidly towards *b*, but leaving a narrow band *b* 2, and then the space between D 5 and F vanishes, *the blue becoming faint and slightly violet.*

N.B. *The blue space F G is most distinctly very violet* before evanescence.

With great dilution the band between b and $b\ 1$ is beautifully distinct.

When the alcoholic solution of chica is mixed with water, its colour and properties are unchanged.

4. *Cudbear*, alcoholic solution, fine deep scarlet. It attacks the spectrum at D and d almost simultaneously, and soon leaves only $b\ D$, or a narrow red. When diluted, it becomes pink, absorbs slightly about $D\ g$, and then the absorption extends almost instantly from D to the violet. The line m is distinctly marked. When diluted with water, it is still bluish pink.

5. *Sulphate of Indigo*, diluted with water, attacks the spectrum at m and n , dividing the red into two by a darkish space between m and n , and then absorbing rapidly the rest of the red towards D , then the violet, and then on both sides up to E , on one side and near F , and on the other side, at great thicknesses, the space $E\ F$, of which $E\ \delta$ is yellow, and $\delta\ F$ blue.

In this case the *yellow* is separated from the *green* by the absorption of the latter, and exists at δ and on the blue space $D\ F$.

At one time $B\ c$, $c\ m$, $m\ n$, and $n\ D$ are all seen as distinct bands.

At a certain thickness this solution is *greenish* blue by daylight, and *bluish pink* in candle-light.

6. *Soda, Sulphate of Chromium*, bright *green* in daylight, and *pink* in candle-light. Its action commences at D and $D\ 2$, absorbing the space D , $D\ 2$, and then on both sides up to near E , and also from the violet end to near F , the *yellow* remaining from E to δ , and the blue from δ to beyond F , almost exactly as in sulphate of indigo. The remaining yellow has more green in it than in sulphate of indigo.

7. *Nitrate of Cobalt*, pale crimson-red by daylight, and (—) in candle-light. It attacks the spectrum almost simultaneously from b to near F , leaving a dark green, and then it attacks it about halfway between F and G , rendering darker and bluer the space from $F\ 34$ to F , and leave $F\ 34$ to G a *whitish blue* band, the absorption having advanced from $F\ 34$ to F . The band $F\ 34$ to G and the violet beyond G are left, and the absorption is complete between $F\ 34$ and towards D , to which it gradually approaches.

8. *Litmus*, dissolved in water, a beautiful *blue* in daylight, and *pink* in candle-light. It attacks the spectrum at m , n , D , and $D\ 2$, and at c , absorbing beautifully the band $m\ D\ 2$, so that at one time, before that band vanishes, we see the luminous portions between these lines about to disappear. At the thickness which does this, all the other parts of the spectrum are unchanged. At greater thicknesses it absorbs the rays towards b , then the violet, leaving much green, yellow, and blue.

9. *Sulphate of Cobalt*, pale pinkish-red by daylight, paler and bluer than the nitrate of cobalt. *It converts the blue into violet*, absorbing from E up to D 4, leaving that space a dark green, and proving that blue rays exist at that place. The space D, D 4 becomes a greenish whitish band.

At greater thicknesses the dark band F, D 4 becomes black, the violet remaining quite distinct.

10. *Sulphate of Ammonia and Chromium*, pale greyish blue by daylight, and pink by candle-light. It has a very curious action on the spectrum. It attacks it most powerfully at the violet end, then at D 2 and D, till the space D, D 2 disappears, which takes place before the absorption has reached E. The absorption advances to n ,—the lines C, c , and m , particularly C, being widened. At great thicknesses a green and blue band, in the centre of which is F, remains, and a bright narrow band C n , which is persistent. The red space is curiously acted upon.

11. *Chromate of Ammonia*, an intense yellow at small, and orange at great thicknesses. It begins its action at the violet end, and advances almost simultaneously over the blue and gradually up to D 2. At greater thicknesses it would doubtless advance far into the red space.

12. *Sulphate of Cobalt* mixed with *Chromate of Ammonia* has nearly the same action on the spectrum as the chromate alone.

13. *Sulphate of Potash and Nickel*, very pale bluish green, attacks the spectrum at B, c , and m , leaving B c and $c m$ distinct bands. It attacks the violet also.

14. *Carbazotate of Potash*, a brilliant intense yellow, the same nearly at all thicknesses. It begins its action at the violet, and absorbs gradually up to δ , leaving violet at the edge before it comes to F.

15. *Red Prussiate of Potash*, a fine intense yellow. The crystals are scarlet. It has almost the same action on the spectrum as carbazotate of potash.

16. *Sulphate of Potash and Copper*, a pale blue. It attacks the violet, and then absorbs from B to c .

17. *Bi-vanadate of Ammonia*, a pale yellow. It acts upon the violet and yellow simultaneously.

18. *Chloride of Iridium and Potassium*, yellow at small, and scarlet homogeneous red at great thicknesses. It begins by a powerful action almost simultaneously at δ and F, when the diluted fluid is almost colourless. It then absorbs δ F. The blue on the side of G towards F then disappears, leaving violet, and a blue band between F and F 3. The absorption advances to b , and when near E the blue disappears, and the spectrum is attacked at D and D 2, the space between these lines disappearing, and the absorption advancing on both sides destroying all the

green and approaching to *n*. A faint band remains at *mn*. Absorption then takes place at B, and C *m* or B *m* is the persistent red.

19. *Carbazotic Acid* is more intensely yellow than the carbazotate of potash. It begins to act upon the spectrum at the violet, and then absorbs up to *b*.

20. *Ammoniuret of Nickel*, a fine deep blue by daylight, and pink by candle-light. It attacks the spectrum at D and D 2, absorbing the space between them, the absorption advancing on each side of D, but deepening the whole red space till it nearly disappears, and leaving only the green and the blue with scarcely any of the violet.

21. *Nitrate of Copper*, greenish blue by daylight, and bluish green by candle-light. It attacks the red space generally, beginning chiefly at the extremity, and advancing to D, then to D 2, *leaving the yellowish green more yellow*, and also all the blue and almost all the violet. The blue is whiter, and the green space is yellower, indicating that the red has been partially absorbed throughout much of the spectrum.

A weak solution first absorbs B C or B *c* sharply, and also widens *m*.

22. *Nitrate of Nickel*, a pale muddy green, but yellowish at considerable thicknesses. It attacks the red space at its extremity, and at the same time the other end of the spectrum up to D 4, leaving the yellowish white band with green on one side and red on the other. It absorbs blue at small thicknesses, *leaving violet in place of the blue*.

23. *Ammoniuret of Copper*, fine deep blue in day- and candle-light. It attacks the red and green, generally beginning at the red end, and absorbing up to F, leaving the blue and violet.

24. *Muriate of Copper*, pale green. It attacks the red space at its extremity, and absorbs the violet.

25. *Sulphate and Nitrate of Iron*, orange-yellow. It attacks the spectrum at the violet end, and advances gradually towards D 4.

26. *Chromate of Potash* (not neutral), intense yellow. It begins at the violet end, and advances gradually up to *b*.

27. *Oxalate of Chromium and Potash*. I have fully described the properties of this remarkable salt in the *Philosophical Transactions*, 1835, p. 91. It has a specific action upon a red ray between A and B, its place being such that $BX = \frac{1}{3} Ba$. It is a sharp and narrow black band, forming a fixed line in all artificial lights, and of great practical value in measuring refractive powers.

28. *Arsenite of Copper in Muriatic Acid*, a fine green. It absorbs the red and violet ends of the spectrum, leaving the red from D to C a *palish flesh or salmon-colour*.

In examining the preceding observations, it will be seen that many of them indicate the compound nature of the spectrum, as explained in my paper "*On a New Analysis of Solar Light*"*. One of the most curious is the *conversion of the blue space into violet*, indicating that the red rays, which I suppose exist to a small extent in the blue space, have been less absorbed than the blue, and consequently that the blue space has thus become violet.

M. Bérard, who does not adopt my views, admits this important fact, but asserts that blue becomes violet by merely diminishing the intensity of its light. To me it appears a most extraordinary opinion that a new colour should thus arise, and that red rays not previously seen should appear, or be created, in the blue space by merely reducing its light! and I cannot suppress the opinion that M. Bérard should have sought for some probable explanation of his fact, if it be one, without regarding it as incompatible with the compound nature of the blue space. I have *proved* by direct experiment, that *in lights of a certain feeble intensity the retina is more sensible to red than to blue rays*, and hence we have a reasonable explanation of the violet colour in the blue space at certain feeble intensities of the light.

I do not avail myself, however, of this explanation. I deny the fact that in my observations the blue became violet by diminishing the intensity of its light, and here is the proof drawn from the observations themselves. In the greater number of them *the blue was not converted into violet*, which it ought to have been in diminished light. The conversion took place in a much smaller number of observations, not because the light was diminished, but because the coloured medium absorbed the blue rays and left the violet. As all my observations were made in light of feeble intensity, the same result should have been obtained in every case where the blue space was left in the spectrum.

Allerly, November 15, 1862.

LXI. *On the Definition of the Temperature of Bodies, and on its Measurement by Thermometers.* By Professor POTTER, A.M.†

THOUGH we continually meet with the popular definition of the temperature of bodies, or their sensible heat, as indicated by instruments called thermometers, yet its scientific definition is not readily found. M. Fourier, as far as I am aware, is the only philosopher who has attempted to give a

* Edinburgh Transactions, 1831, vol. xii. p. 123.

† Communicated by the Author.

strictly scientific definition of the measurements of both caloric and temperature, and the latter only imperfectly. At page 20 of his *Théorie Analytique de la Chaleur*, he defines the *unit of caloric* to be the quantity which will convert a unit of mass of ice at the freezing-point into water at the same temperature; and if a number C of these units will raise a unit of mass of any body or substance through a temperature from 0 to 1, then C is the specific capacity for heat of that substance; and if a quantity of caloric zC (where z may be positive or negative) is communicated to the body, then z is the *temperature* communicated to it, and is *generally* proportional to the change of the volume of the body, but not universally.

When we come to great ranges of temperature, we shall see that the above definition of their measurement is not complete. Now we have two mathematical expressions which involve temperatures, and which have received a general assent as to their accuracy, from which we may deduce the definitions which have been tacitly assumed in their formation, and which we may call the *statical* and *dynamical* definitions of the measurements of temperatures. The first, furnishing the statical definition, is the formula by which the specific heat of bodies is calculated; and the second, giving the dynamical definition, is the formula for the velocity of cooling of bodies in a vacuum.

We define the specific heat of a body, or its capacity for caloric, to be measured by the quantity of caloric necessary to be communicated to, or abstracted from, a unit of mass of the body in order to raise or lower respectively its temperature one degree. Putting c for this capacity, then the quantity which will raise a mass m through one degree is $m \cdot c$, and the quantity which will raise the mass m through t degrees is $m \cdot c \cdot t$, for which we put Γ , or

$$\Gamma = m \cdot c \cdot t$$

$$\therefore t^{\circ} = \frac{\Gamma}{m \cdot c}$$

where it is supposed that c has not changed sensibly in the interval t° of temperature.

If we put γ for Γ when $t^{\circ} = 1^{\circ}$ and $m = 1$, we have

$$1^{\circ} = \frac{\gamma}{c}$$

Now c is found to be different for different substances at the same temperature, and to vary for the same substance at different temperatures; and γ must vary directly as c varies. As the capacities of bodies for caloric are found to increase with their temperatures, then at very low temperatures, where c may be

very small, we have γ very small also. When we recognize that c is a function of the temperature, say $c=f(t^\circ)$, the correct differential expression becomes

$$d(t^\circ) = \frac{d\gamma}{c}$$

or

$$d\gamma = f(t^\circ) dt^\circ$$

and γ is now the quantity requisite to raise the unit of mass through 1° , and therefore

$$\gamma = \int_t^{t+1} f(t^\circ) dt^\circ$$

With respect to the dynamical definition of the measure of temperatures, we have the formula of MM. Dulong and Petit as follows. Let v be the velocity or rate of cooling of a body in a vacuum, $(\theta + t)$ the temperature of the body, θ the temperature of the substance surrounding the vacuum, a and m constants, and

$$M = m \cdot a^\theta$$

then

$$v = M(a^t - 1)$$

They say*, "We may conclude, then, that if it were possible to observe the absolute cooling of a body in a vacuum, that is to say the loss of heat of a body, without any restoration on the part of the surrounding bodies, this cooling would follow a law in which the velocities would increase in a geometrical progression, while the temperatures increase in an arithmetical progression; and, further, that the ratio of this geometrical progression would be the same for all bodies, whatever the state of their surface may be." In their experiments the ratio was 1.0077 for all bodies, and $M = m \cdot a^\theta$ is the return radiation of the surrounding body; and in order that this may be zero, we must have $\theta = -\infty$, or the absolute zero of temperature must be at an infinite number of degrees below the freezing-point of water; and yet, as they argue, M may not be zero, since the capacities of bodies diminish as their temperatures diminish.

The expression for the rate of cooling becomes, if τ is the time,

$$v = -\frac{dt^\circ}{d\tau} = M \cdot a^{t^\circ}$$

or, taking finite differences for differentials,

$$-\delta t^\circ = v \cdot \delta \tau = M a^{t^\circ} \cdot \delta \tau$$

* Herschel 'On Heat,' p. 280.

450 Prof. Potter *on the Definition of the Temperature of Bodies*,
and if $-\delta t^\circ = 1^\circ$, we have

$$1^\circ = Ma^{t^\circ} \cdot \delta\tau$$

and the time $\delta\tau$ becomes longer as t° is less.

The full discussion of these formulæ was not carried out by MM. Dulong and Petit; but, remembering the principle of *the homogeneity of algebraic expressions applied to physics*, we may easily put them into an intelligible form.

Taking the formula $v = Ma^{t^\circ}$, let v_0 be the value of v when $t^\circ = 0$, and then

$$v_0 = M$$

$$\therefore v = v_0 \cdot a^{t^\circ}$$

and since a and t are numbers, the equation is homogeneous, and may be put into a form more intelligible, since v_0 would be unknown.

Let $t^\circ = \theta + t'^\circ$, and v' the value of v when the temperature was θ , or

$$v' = v_0 \cdot a^\theta$$

then

$$\frac{v}{v'} = \frac{v_0 \cdot a^{(\theta + t'^\circ)}}{v_0 \cdot a^\theta} = a^{t'^\circ}$$

and

$$v = v' \cdot a^{t'^\circ}$$

and the law holds good for any assumed zero of temperature; also if we put $t'^\circ = 1^\circ$,

$$\frac{v - v'}{v'} = (a - 1) = \text{constant}$$

for any temperature, and *the change* in the rate of cooling is uniform.

Taking the logarithms, we have

$$t'^\circ \log_\epsilon (a) = \log_\epsilon \left(\frac{v}{v'} \right)$$

and

$$t'^\circ = \frac{\log_\epsilon \left(\frac{v}{v'} \right)}{\log_\epsilon (a)}$$

which expresses the temperature t'° for any assumed zero in terms of the velocities of cooling by radiation only; and the degrees above zero equal the logarithm of the ratio of the velocity of cooling at that temperature to the velocity of cooling at the zero of the scale divided by a constant quantity. This is the dynamical measure of the temperature t'° on the scale; and v , the

velocity of cooling, equals the number of degrees lost in a unit of time, considered constant through that time.

By equating the value of t'° to the value of t° before found, we have

$$\frac{v}{v'} = e^{\frac{\Gamma}{mc} \cdot \log_e a}$$

for any interval through which c might be taken constant.

Though these expressions do not immediately guide us in our search for a normal thermometer, yet they are of the greatest use in pointing out the errors we have to avoid when empirical results and approximate laws are discussed.

In the first instance, the scales of degrees of both the air and liquid thermometers would naturally be formed in series at equal intervals, and the theory of a correct scale would only come under discussion when discrepancies between the different instruments became evident. De Luc found a discrepancy in the scales of the air and mercurial thermometers*, and concluded that, if a mass of mercury expanded 29 parts between the freezing- and boiling-points of water, then the expansion was 14 parts in the first half of the scale, and 15 parts in the last half. Dalton thought the irregularity in the expansion of mercury considerably greater than as stated by De Luc, and proposed a new scale for the mercurial thermometer, for which a Table is given in Henry's 'Chemistry.' In the Appendix, however, to part I of vol. ii. of his 'New System of Chemical Philosophy,' published in 1827, he says he then thought the differences between the air and mercurial thermometers to be less than he formerly concluded them to be.

From Gay-Lussac's empirical law for the expansion of the gases, the air thermometer has been considered the normal thermometer; and Dulong and Petit accept this, although it leads to consequences at variance with their experiments on the cooling of bodies.

M. Regnault, from more recent researches, concludes that the air thermometer must lose its position of a normal thermometer and take its place with the others as an abnormal one.

When we come to review the whole subject as discussed by the many eminent men who have written upon heat, we find continual use of the expression *the uniform expansion of some body* which might be employed to form a normal thermometer. Now it appears that we must find out, in the first place, what is the true mathematical expression for *uniform expansion*, before we can be expected to arrive at a true thermometric scale of degrees.

* Henry's 'Chemistry,' vol. i. p. 81.

The discrepancies which were found between the scales led to the conclusion that the divisions into equal intervals could not apply accurately to all, though it has been taken to involve uniform expansion. Thus if V is the volume of a body at *any given* temperature, and δV the increase of volume for each degree of temperature, then $\delta V = \text{constant}$ was supposed to involve uniform expansion. This is undoubtedly incorrect; for the expansion of a body of volume V is the ratio of the increment of V to V ; or putting δV for the increment, then the expansion is $\frac{\delta V}{V}$; and if the expansion is constant or uniform, then $\frac{\delta V}{V} = \text{constant}$ for all temperatures. This not being hitherto recognized, we have had inadequate discussions in the search for a normal thermometer.

As to the probability of gases, spirit, mercury, or any other fluids being subject to uniform expansion, it is desirable to consider the physical state of each in the first instance. In the liquids under the pressure of their vapours only at their surfaces, their volume is that at which the repulsive force due to their inherent caloric balances the attraction of aggregation of their atoms. At certain temperatures peculiar to each substance, the attraction of aggregation is overbalanced by the repulsion due to the caloric of the body, and it ceases to be of two kinds, liquid and vapour, and consists of vapour only. We see that the law of the expansion of liquids depends on the increase of the repulsive force due to the caloric of the body and the diminution of the attractive force of aggregation. We cannot see *à priori* that the law of expansion for increase of temperature should be very simple.

With respect to gases under constant or uniform pressures, the consideration of their volumes involves the law of their elastic force with respect to their density (or reciprocal of their volume) at different temperatures. Then, if the elastic force of the gas is constant, the repulsive force due to the combined caloric determines the volume and density of a given mass. We cannot here, again, from *à priori* reasoning, see the law of expansion for increase of temperature. It has been generally called uniform expansion; but, with the exception of Dalton, uniformity of expansion has been universally taken to mean expansion by equal increments of volume for equal increments of temperature, as expressed in the law of Gay-Lussac for the relation of the volumes and temperatures of gases under constant pressures, or $V = V_0(1 + \alpha t^\circ)$, where V_0 is the volume of the gas at the freezing-point of water, and V its volume at t° above that point. If this were a physical law, the absolute

zero of temperature would be found by making $1 + \alpha t^\circ = 0$, or $t^\circ = -\frac{1}{\alpha}$, which is not in accordance with the results of Dulong and Petit, whose law of the cooling of bodies in a vacuum gives the absolute zero of cold at minus infinity of degrees below the freezing-point of water. According to Dalton's view, the correct law of uniform expansion exists in gases; or, for them, for each change of a degree of temperature we have $\frac{\delta V}{V} = \text{constant}$; and putting this into a differential form, $\frac{dV}{V} = \alpha dt^\circ$, which, by integrating and correcting the integral, gives $V = V_0 \cdot e^{\alpha t^\circ}$, and the degrees t° may be counted \pm from any starting-point at which the volume of the gas is V_0 . The volume V becomes zero now only when $t^\circ = -\infty$, or at the absolute zero of Dulong and Petit. It is clear that the science of Molecular Physics is not yet sufficiently advanced to assist us in discriminating the true law of the expansion of gases for increase of temperatures independently of experiments.

For any bodies whatever, to find the volume V at a temperature t° , when the volume V_0 at the commencement of the scale and the constant α are known, we have the formulæ

$$\text{for expansion by equal increments } V = V_0(1 + \alpha t^\circ)$$

$$\text{for uniform expansion } \dots \dots V = V_0 \cdot e^{\alpha t^\circ}$$

Now for mercury the most careful experiments were those of MM. Dulong and Petit*, who found that the expansion of mercury between the freezing- and boiling-points of water was $\frac{1}{55.50}$ th part, for the next equal increase of temperature it was $\frac{1}{54.25}$ th part, for the next equal increase again it was $\frac{1}{53.00}$ rd part, the two second intervals being subject to any irregularity the air thermometer possessed by which they were measured. It is clear that the expansion of mercury is not by the law of equal increments, which has puzzled De Luc, Dalton, and others; but it is, we shall find, conformable to the law of uniform expansion, and, when properly graduated, the mercurial thermometer approximates exceedingly near to a normal thermometer—the proper correction for the expansion of the containing glass bulb and tube being employed.

Let V_0 be the volume of any mass of mercury at the freezing-point of water, V_1 its volume at the boiling-point, V_2 its volume at another equal increase of temperature above the boiling-point, V_3 its volume at another equal increase of temperature above the last.

* *Annales de Chimie et de Physique*, 1817.

Then, having given $V_1 = V_0 + \frac{V_0}{55.50}$, we ought to be able to calculate V_2 and V_3 , if the expansion is uniform, from the formula $V = V_0 \epsilon^{\alpha t^\circ}$. Then putting unity for the degrees between the freezing- and boiling-points of water, or $t^\circ = 1$, we have given

$$V_1 = V_0 \cdot \epsilon^\alpha = V_0 \left(1 + \frac{1}{55.5} \right)$$

$$\therefore \alpha = \log_\epsilon \left(\frac{565}{555} \right) = .0178576$$

and

$$V_2 = V_0 \cdot \epsilon^{2\alpha} = V_0 \epsilon^{.0357152}$$

or

$$.0357152 = \log_\epsilon \left(\frac{V_2}{V_0} \right) = \log_\epsilon (1.03636)$$

and

$$V_2 = V_0 (1.03636)$$

$$V_1 = V_0 (1.018018)$$

$$\therefore \frac{V_2 - V_1}{V_0} = .018342 = \frac{1}{54.52}$$

which is a little smaller, but very near $\frac{1}{54.25}$, found by Dulong and Petit with the air thermometer as ordinarily graduated.

Again,

$$V_3 = V_0 \cdot \epsilon^{3\alpha} = V_0 \epsilon^{.0535728}$$

or

$$.0535728 = \log_\epsilon \left(\frac{V_3}{V_0} \right) = \log_\epsilon (1.05503)$$

and

$$V_3 = V_0 (1.05503)$$

$$V_2 = V_0 (1.03636)$$

$$\therefore \frac{V_3 - V_2}{V_0} = .01867 = \frac{1}{53.56}$$

which, again, is a little smaller, but very near $\frac{1}{53.00}$, the quantity found by Dulong and Petit with the air thermometer.

That liquid mercury is subject to the law of uniform expansion, and its volumes are in a geometrical progression for increments of temperature in arithmetic progression, is evident; but it is an empirical result only. We cannot expect the compound liquids, as water, alcohol, ether, &c., to follow the same law

à priori; and indeed *water* is known to form a remarkable deviation from such a law, having a maximum density at $39^{\circ}\cdot 1$.

The next consideration of consequence is the true law for the expansion of gases; and the generally received law of equal increments leads, as before stated, to inadmissible consequences; so that, taking the mercurial thermometer as our standard, we should examine the applicability of the law of uniform expansion to them, or Dalton's law. Now the formula $V = V_0 \epsilon^{\alpha t^{\circ}}$ gives, by putting $t^{\circ} = 1$ for the temperature between the freezing and boiling of water for atmospheric air, and V_1 the corresponding volume,

$$\frac{V_1}{V_0} = \epsilon^{\alpha} = 1\cdot 375, \text{ according to Gay-Lussac.}$$

$$= 1\cdot 3726, \quad ,, \quad \text{Dalton.}$$

$$= 1\cdot 3645, \quad ,, \quad \text{Rudberg.}$$

$$= 1\cdot 3665, \quad ,, \quad \text{Regnault.}$$

Taking Rudberg's determination, we have

$$\alpha = \log_e (1\cdot 3645) = \cdot 310788$$

and the volume V of atmospheric air is found from the formula

$$V = V_0 \cdot \epsilon^{t^{\circ} \times \cdot 310788}$$

where V_0 is the volume at *any temperature*, and t° are the \pm degrees above or below it respectively, measured in terms of the interval between the freezing- and boiling-points of water taken as unity. If t° be measured on Fahrenheit's scale, we have $\frac{t^{\circ}}{180}$

in place of t° , and

$$V = V_0 \cdot \epsilon^{t^{\circ} \times \cdot 0017266}$$

If the t° are on the Centigrade scale, we have $\frac{t^{\circ}}{100}$ in place of t° ,

and

$$V = V_0 \cdot \epsilon^{t^{\circ} \times \cdot 00310788}$$

For mercury, from the above, we have for degrees on Fahrenheit's scale,

$$V = V_0 \cdot \epsilon^{t^{\circ} \times \cdot 00009921}$$

and for degrees on the Centigrade scale,

$$V = V_0 \cdot \epsilon^{t^{\circ} \times \cdot 000178576}$$

With these formulæ it is easy to compare the scales of the air and mercurial thermometers when formed on the rules of equal increments and uniform expansion.

The expansion of mercury being less than one-twentieth that of air, the differences of the two scales for the mercurial thermometer are very much less than those of the two scales for the air

thermometer, because the differences between an arithmetic and geometric series having the first and last terms equal become less as the common ratio of the latter is nearer to unity.

To compare the two scales for the mercurial thermometer at the temperature halfway between the freezing- and boiling-points, where the difference is the greatest for that range, we have, from equal increments and $t^{\circ} = 90^{\circ}$,

$$\begin{aligned} V &= V_0(1 + \alpha t^{\circ}) = V_0 \left(1 + \frac{90}{180 \times 55.5} \right) \\ &= V_0 \left(\frac{112}{111} \right) \\ &= V_0 \cdot \epsilon^{\alpha' t'^{\circ}} \\ \therefore \alpha' t'^{\circ} &= \log_{\epsilon} \left(\frac{112}{111} \right) \end{aligned}$$

whence

$$t'^{\circ} = \frac{.0089687}{.00009921} = 90^{\circ}.40$$

and the difference from 90° is $0^{\circ}.40$, or less than $\frac{1}{2}$ degree. M. Regnault concluded from his experiments* that the ordinary mercurial thermometer had an error of $0^{\circ}.2$ Centigrade, or $0^{\circ}.36$ Fahrenheit, by comparison with the air thermometer graduated for equal increments at this temperature.

Since mercury is admitted not to expand according to the law of equal increments, we are not entitled to compare the higher temperatures on the two scales; but the differences are found to be small, and we may conclude that the mercurial thermometer, graduated according to the law of uniform expansion, is very nearly, if not accurately, a normal thermometer.

Hence in future the expansion of the gases must be tested by the mercurial thermometer correctly graduated for uniform expansion, and not the converse, as has hitherto been done; and Gay-Lussac's law must take its real place as an approximate empirical law only.

The general formula which must replace that of Amontons, when we adopt the principle of uniform expansion in gases, is

$$p = \kappa_0 \cdot \rho \cdot \epsilon^{\alpha t^{\circ}}$$

where $\kappa_0 = \frac{p_0}{\rho_0}$, when $t^{\circ} = 0$ at the commencement of the thermometric scale; and the calculations are very easy and concise when we have a table of hyperbolic logarithms.

* *Annales de Chimie et de Physique* for 1842, p. 98.

LXII. *Note II. on some Objections of Mr. Cayley and Mr. Cockle.*
By G. B. JERRARD*.

1. **I**N the October Number of this Magazine Mr. Cayley and Mr. Cockle renew their objections. The former of these mathematicians there says:—"Mr. Jerrard, in his paper, 'Note on some Objections of Mr. Cayley and Mr. Cockle,' in the September Number, p. 193, concludes, so far as relates to me, as follows:—"The curious irrelevancy of Mr. Cayley's objection will now be seen. He merely occupies himself in proving what I had taken for granted, while he leaves untouched the main question to be discussed, which is this:—Is Lagrange's theory inapplicable to the case of $u=\alpha$, $v=\alpha^5$, in virtue of the equation $(e'_1)?$ " "

The last sentence, I may just observe in passing, was by me put in italics. That the additional emphasis thus given to the point in dispute was not quite needless we shall see presently.

2. It will be remembered that

$$\alpha = f(\iota) f(\iota^2) f(\iota^3) f(\iota^4),$$

and that the equation of definition for $f(\iota^n)$ is

$$f(\iota^n) = x_1 + \iota^n x_2 + \iota^{2n} x_3 + \iota^{3n} x_4 + \iota^{4n} x_5;$$

in which x_1, x_2, \dots, x_5 denote the five roots of

$$x^5 + A_1 x^4 + A_2 x^3 + \dots + A_5 = 0,$$

and $\iota, \iota^2, \iota^3, \iota^4$ the imaginary roots of the binomial equation

$$\rho^5 - 1 = 0.$$

The function here represented by α is a remarkable one. It possesses, as is well known, the surprising property of having the same number of values as its fifth power. If, however, we resolve α and α^5 into their component parts,

$$\begin{array}{cccc} f(\iota), & f(\iota^2), & f(\iota^3), & f(\iota^4), \\ [f(\iota)]^5, & [f(\iota^2)]^5, & [f(\iota^3)]^5, & [f(\iota^4)]^5, \end{array}$$

and compare one of these parts, say $f(\iota)$, in α with the corresponding part, $[f(\iota)]^5$, in α^5 , we shall see that the homogeneity of α and α^5 is not such as to penetrate and pervade these functions. Extrinsically α and α^5 are homogeneous; but intrinsically—at least in relation to some of their corresponding parts—they are not so.

As for $u, v, (e'_1)$, everything relating to them is, in my paper for September, explained thus:—

i.† "Let us suppose, in conformity with my previous notation

* Communicated by the Author.

† i. ii. iii. iv. are substituted in the text for 2, 3, 4, 5 respectively in the original paper, to avoid the clashing of two sets of numerals belonging to the same system.

for $n=6, m=5^*$, that u and v designate two rational six-valued homogeneous functions of the roots of the equation

$$x^5 + A_1 x^4 + A_2 x^3 + \dots + A_5 = 0;$$

then by Lagrange's theory we can generally express either of these functions in rational terms of the other and of the coefficients A_1, A_2, \dots, A_5 . The only exception, according to that theory, is when there are equal roots in (U) or (V), the equations on which u, v respectively depend,—or rather when the number of unequal roots in (U) differs from that of unequal roots in (V).

ii. "But I maintain that when u and v are taken equal to those particular six-valued functions represented by

$$\alpha, \alpha^5,$$

the theory of Lagrange will afford us no aid whatever in establishing a rational communication between them, although in this case the equations (U), (V) have undoubtedly no equal roots. Vain, I assert, must every attempt be to proceed beyond

$$\alpha^5 = (\alpha)^5.$$

iii. "That there is something exceptional in the case in question, we may see at once without entering into the calculations.

"For beside the equation

$$v = \mu_5 + \mu_4 u + \mu_3 u^2 + \dots + \mu_0 u^5, \quad \dots \quad (e_1)$$

for expressing v in rational terms of u ,—which equation in ordinary cases would, if there were no equal roots in (U), (V), be sufficient for the purpose intended,—we have, in the present instance,

$$v = u^5. \quad \dots \quad (E_1)$$

Does (E₁), I would ask, exert no disturbing influence on the coefficients of (e₁)?

"Again, in ordinary cases we can generally obtain a single definite equation of the form

$$u = \nu_5 + \nu_4 v + \nu_3 v^2 + \dots + \nu_0 v^5; \quad \dots \quad (e_2)$$

but here we set out with supposing that

$$u = \sqrt[5]{v}; \quad \dots \quad (E_2)$$

that is to say, u is to be both a rational and an irrational function of v .

"What wonder, therefore, if, in a case so anomalous, Lagrange's theory should furnish not only illusory but even fallacious results. And, indeed, on a closer inspection of the subject, surmise will quickly give place to certainty.

* Philosophical Magazine for May 1861..

iv. "Combining (e_1) , (E_1) , we have

$$0 = \mu_5 + \mu_4 u + \mu_3 u^2 + \dots + (\mu_0 - 1)u^5; \quad \dots \quad (e'_1)$$

in which $\mu_5, \mu_4, \dots, \mu_0$ must all of them, in accordance with what has been stated in art. i., be expressible as rational functions of A_1, A_2, \dots, A_5 .

"But as (U) , the equation on which u depends, is rationally irreducible below the sixth degree, the coefficients

$$\mu_5, \mu_4, \dots, (\mu_0 - 1)$$

of the equation (e'_1) ought severally to vanish.

"When, therefore, u, v are equal to α, α^5 respectively, (e_1) ought to become

$$v = u^5,$$

which is precisely identical in form with the equation (E_1) itself.

"We thus perceive that we cannot derive any aid from Lagrange's theory in expressing α^5 as a rational function of α and of A_1, A_2, \dots, A_5 . It remains to show that (e_2) also is inapplicable when $u = \alpha, v = \alpha^5$.

"The equation (e_2) is supposed to be obtained directly from the theory of Lagrange. But an equation identical in its ultimate form with (e_2) ought to be found by combining (e_1) and (U) . Whence

$$u = \frac{0}{0}; \dots (e_1, U)'$$

if

$$u = \alpha, \quad v = \alpha^5,$$

since (e_1) here becomes, or at least ought to become, $\alpha^5 = \alpha^5$.

"Of the interpretation of $(e_1, U)'$, which is indicative of an incongruity, I intend to speak in another place."

From this extract it will be perceived that (e'_1) expresses, by means of mathematical symbols, the exceptional nature of the homogeneity existing between α and α^5 .

3. How then does Mr. Cayley answer the question, "*Is Lagrange's theory inapplicable to the case of $u = \alpha, v = \alpha^5$, in virtue of the equation (e'_1) ?*"

"But if," remarks Mr. Cayley—shunning the true point in dispute, and not giving due heed, as we shall see, to the definition of the word *objection*,—"but if my objection be (curiously or otherwise) *irrelevant*, then the proposition I contend for might be admitted without prejudice to Mr. Jerrard's results: this proposition is, that Lagrange's theory is applicable to the case of $u = \alpha, v = \alpha^5$, which is a case not excluded by the only exception (the case of equal roots) to the general theory, and therefore, notwithstanding the equation (e'_1) or anything else whatever, coming within the general theory. Mr. Jerrard, in his reply,

contends for the contradictory proposition that, in virtue of the equation (e'_1) , Lagrange's theory is not applicable to the case $u = \alpha$, $v = \alpha^5$, and he thus in effect treats the objection as a *relevant* one. It appears to me that the objection is not only a relevant one, but that the proposition therein contended for is completely proved; at any rate"—observe this—"the issue is so narrow a one that it seems useless to argue it further, and it is not my intention to do so."

By the word *objection* I have been accustomed to mean, "that which is presented in *opposition*; *adverse* reason or argument." To me therefore it seems as clear as any axiom of Euclid that Mr. Cayley's objection, in which (to use the very words quoted in art. 1) *he merely occupies himself in proving what I had taken for granted, while he leaves untouched the main question to be discussed*, cannot rightly be classed with relevant objections. No matter what the subject in dispute is, an objection characterized as above is, I hold, essentially irrelevant. By mentally supplying the characteristic of Mr. Cayley's objection in every place, the incongruity of his remarks will at once appear.

Just compare my paper in the Philosophical Magazine for May 1861 with Mr. Cayley's for March 1862.

In the first of these two papers it will be seen that, after stating generally the results at which Lagrange had arrived with respect to homogeneous functions, and pointing out (e'_1) as characteristic of the case of $u = ft ft^2 ft^3 ft^4 *$, $v = (ft ft^2 ft^3 ft^4)^5$, I thus conclude:—"We are therefore not permitted to assume that the resolvent product can in general—that is, when U has no equal roots—be expressed rationally in terms of its fifth power.

"Again, it is generally possible to establish a rational communication between that fifth power and the function W †, as is evidenced in this latter case from the non-existence of any illusory equation corresponding to (e'_1) .

"We are thus furnished, as will be seen, with a new confirmation of the validity of my method of solving equations of the fifth degree."

Here, then, the controversy ought to have ended. In Mr. Cayley's attempt to fasten a contradiction on my method, Lagrange's theory had given way. That theory cannot bind (e'_1) . No stronger confirmation of the validity of the method in question can easily be imagined than this.

Let us now turn to Mr. Cayley's paper for March. From the

* The function afterwards denoted by a , called also the resolvent product.

† The function W depends on the final equation of the sixth degree, discussed in my 'Essay on the Resolution of Equations.' Published by Taylor and Francis, Red Lion Court, Fleet Street, London.

tone of his remarks we are led to infer that he is opposing a conclusion of mine with respect to α and α^5 . But what does he say of (e'_1) ? Not one word. The very existence of that equation is ignored by him throughout. I can only marvel at what I have not inaptly, I think, termed *the curious irrelevancy of his objection*.

In his paper for October he does indeed hazard the assertion that "notwithstanding the equation (e'_1) or anything else whatever," Lagrange's theory is applicable to the case of $u=\alpha$, $v=\alpha^5$; but a moment's reflection on what has been demonstrated in art. 2 will convince us of the character of Mr. Cayley's assertion.

4. In recklessness of assertion, however, Mr. Cayley is far outdone by Mr. Cockle. What, for instance, shall I say of the concluding statement of the latter mathematician in his separate paper for October?—"In employing Lagrange's theory we may confine our attention to (E_1) , and need not concern ourselves about (e'_1) in any way." What assertions are there involved in his alleged proof of this strange proposition*? It is amazing to see how completely the human intellect may be overmastered by prejudice.

Lesson.

In rescuing with such pains and difficulty from the class of impossible problems the noted problem of equations, a lesson (which I wish not to put away from me) presses itself upon my mind.

If in a question in mathematics which comes wholly within the province of our reason, we are, in fixing the limits of possibility, thus liable to fall into an error, glaring when pointed out, and yet so wide-spread as almost to overbear opposition, what infant's weakness is it, in "The mysterious scheme of things in the midst of which we find ourselves†," that stretches beyond us on every side in boundless immensity, to take as sure standards of impossibility *the things which are impossible with men*.

The lesson is a humbling one; but it is not less instructive on that account.

November 1862.

* Figure to yourself a geometrician following an analogous method to that pursued by Mr. Cockle, and disputing the validity of the proof of the sixth proposition of the first book of Euclid's 'Elements' on such grounds as these:—

Denoting by \mathcal{E} a triangle, the angles at the base of which are equal, then, if the sides be not equal, we can cut off towards the base of the triangle a second triangle denoted by ϵ , which shall be equal to the first or given one; but we must not draw any inference from a comparison of \mathcal{E} and ϵ : as we may confine our attention to \mathcal{E} , and need not concern ourselves about ϵ in any way. This will perhaps enable you to realize the curious nature of the statement mentioned in the text.

† See Butler's 'Analogy,' part 1, conclusion.

LXIII. *Explanations of Phenomena of Light on the Hypothesis of Undulations.* By PROFESSOR CHALLIS, M.A., F.R.S., F.R.A.S.*

THE explanations I am about to propose rest on the hypothesis that the phenomena of light are visible effects of the motions and pressures of a continuous elastic fluid the pressure of which is proportional to the density. This hypothesis brings the facts to be explained into immediate connexion with the hydrodynamical theorems of which I have given demonstrations in the Numbers of the Philosophical Magazine for June, August, and October. The references to the theorems will be made by means of the articles, numbered from 1 to 48, which make up the three communications. In the instances of several of the more common phenomena, the theoretical explanations are so obvious that little more is required than merely referring to the articles. With respect to others, it will be necessary to introduce some special considerations. It is, however, to be understood that, whether the explanations be obvious or not, the truth of the hydrodynamical theorems rests on principles and reasoning altogether independent of this application of them, and that they receive no additional confirmation from the success with which they explain phenomena, which is only to be taken as evidence of the reality of the æthereal medium and of its assumed properties. I shall at present confine myself to those phenomena which have no particular relations to visible and tangible substances, but depend only on the qualities of the medium in which the light is generated and transmitted. The phenomena of reflexion, refraction, diffraction, &c. are reserved for consideration after the explanations of the other class of phenomena have tested the reality of the hypothetical æther and of its supposed qualities.

One of the most observable and general laws of light is its transmission through space *in straight lines independently of the mode of its generation*. This fact is theoretically explained by the rectilinear axes of free motion of the æther, and by the circumstance that the motion resulting from any kind of disturbance is, to the first power of the velocity, composed generally of vibratory motions relative to such axes. The theorems on which this explanation rests are discussed in articles 11, 20–30, 34, and 38.

The law of rectilinear axes of free motion having been first deduced from the assumed properties of the æther, the mathematical reasoning then conducted to specific analytical expressions for the motions and condensations relative to these axes,

* Communicated by the Author.

antecedently to the supposition of any particular arbitrary disturbance. The axis of z being supposed coincident with an axis of free motion, V being the velocity transverse to the axis at the point xyz distant from it by r , and w being the velocity parallel to the axis, and σ the condensation at the same point, the values of V , w , and σ are given with sufficient approximation for the proposed applications by the following equations:—

$$\frac{d\phi}{dz} = m \sin \frac{2\pi}{\lambda} (\kappa at - z + c), \quad f = 1 - \frac{4r^2}{\lambda^2} \text{ (arts. 42 and 48),}$$

$$V = \phi \frac{df}{dr} = \frac{4mr}{\pi\lambda} \cos \frac{2\pi}{\lambda} (\kappa at - z + c),$$

$$w = f \frac{d\phi}{dz} = m \left(1 - \frac{4r^2}{\lambda^2}\right) \sin \frac{2\pi}{\lambda} (\kappa at - z + c),$$

$$\sigma = -\frac{f}{a^2} \cdot \frac{d\phi}{dt} = \frac{m\kappa}{a} \left(1 - \frac{4r^2}{\lambda^2}\right) \sin \frac{2\pi}{\lambda} (\kappa at - z + c).$$

The distance r from the axis is supposed to be always very small compared to λ (see art. 44), for which reason f is very nearly equal to unity in the expressions for w and σ . That supposition has been made because the investigation of the above equations depends on the integrability of $u dx + v dy + w dz$, and this condition is more strictly fulfilled in proportion as the distance from the axis is smaller (art. 20). It is, in fact, assumed in this theory that that analytical circumstance is the exponent of the physical limitation of a ray of light. Accordingly I now proceed to account for the known properties of individual rays of light by means of the above equations.

The circumstance to be first noticed respecting the expressions for V , w , and σ , is that they are all functions of the quantity $\kappa at - z + c$, and that consequently the velocities and condensations of the undulations which they represent are propagated through space with the constant velocity κa . Now as the theory supposes that by the agency of these undulations the sensation of light is produced, the uniformity of their propagation corresponds to the ascertained fact usually described as *the uniform propagation of light*. This fact is rather the uniform propagation of conditions of physical force by the action of which on the parts of the eye light becomes sensible.

It is proper to state here that the velocity of propagation is the constant κa when only terms of the first order are taken into account; and that if the approximation be carried further, the expression for the rate of propagation involves the quantity m , as is shown by the value of a_1^2 given in art. 31. Unless, therefore, m be the same for different undulations, the rates of

propagation, although uniform, will be different. And even if m should be the same for every primitive series of undulations, its value might be altered (as there will be occasion to point out hereafter) by arbitrary conditions to which the undulations may be subjected. It must therefore be admitted as a theoretical result, that different undulations are propagated with uniform velocities slightly different. The relative effect of this circumstance would be taken into account by supposing the quantity c to vary slowly with the time. I am not aware that the explanations of any phenomena of light would be thereby affected.

Next, it may be remarked that as the expressions for the velocities and condensation contain the constant factor m , and as the dynamical effect of the æthereal undulations results from their velocity and pressure, the amount of such effect must depend upon the value of that factor, which may consequently be regarded as the symbolical exponent of the quality of light named its *intensity*. We shall hereafter have occasion to speak of expressions for the amounts and variations of luminous intensity; at present a few preliminary remarks may be made relative to the circumstances under which the intensity is initially determined.

It must be concluded from experiment, as will be fully shown in a subsequent part of this communication, that the sensation of light is in no degree due to the velocity w , but depends exclusively on the transverse velocity V . This must be accepted as an experimental indication, which it does not belong to the part of the undulatory theory of light now under consideration to account for, the fact depending on dynamic relations between the æther and the constituent atoms of the eye. In like manner we must conclude from experiment that light is produced by transverse movements of the æther within distances from the axis very small compared to λ , it being a known fact that spaces very much narrower than the breadth of an undulation have been made visible by powerful microscopes. This limitation, as we have already seen, corresponds to an analytical circumstance, which, however, does not of itself explain why the more distant movements are inoperative in regard to light. The two limitations being admitted, the analytical expression of the transverse accelerative force which, by acting on the molecules of the eye, excites the sensation of light, will be

$$-\frac{a^2 d\sigma}{dr}, \text{ or } \frac{8m\kappa ar}{\lambda^2} \sin \frac{2\pi}{\lambda} (\kappa at - z + c),$$

the value of r being very small compared to λ . Hence this force for every separate ray varies as the distance from the axis. Consequently for a bundle of rays the same law, as is known, would

hold good with reference to an axis passing through the centre of gravity of all of them. Possibly this circumstance may be the physical reason that the above accelerative force is effective only so far as it varies as the distance from the axis, which law is more exactly true in proportion as the distance from the axis is less. It is also worthy of remark that the above expression contains λ^2 in the denominator of its factor. Nothing, however, respecting the intensity of the sensation of light can be inferred from this circumstance, since we do not even know why the eye is insensible to vibrations for which the value of λ is not included within certain limits. All that we can assert is that, *cæteris paribus*, the intensity of an individual ray is dependent on the constant m .

The circular functions which are factors in the expressions for V and σ indicate regular periodicity in the dynamical effects of the vibrations; and as we know from experiment that such periodicity in respect to sound corresponds to the sensation of the pitch of a musical note, we may conclude analogically that regular periodic vibrations of the æther have the effect of producing the sensation of *colour*. The kind of colour depends on the number of vibrations in a given time, which again depends on the value of λ and that of the absolute constant κa . Consequently the symbol λ , originating in *à priori* reasoning applied to the supposed æther, may be regarded as the exponent of colour.

So also the symbol c , the value of which depends on the velocity w at a given point of the axis at a given time, corresponds to a physical reality named the *phase* of a vibration, the meaning of which will be best understood by what will presently be said respecting the interference of vibrations.

The *mutual independence of rays of light*, exhibited by the fact that the same parts of space may be simultaneously traversed by rays from different origins without any perceptible disturbance of each other, is at once and satisfactorily explained by the law of the coexistence of small vibrations. This law applies to the æthereal undulations of the present theory, because the equations which express their properties were deduced from linear differential equations with constant coefficients. See arts. 22, 26, and 38.

The same law of the coexistence of small undulations serves to explain the observed *interference* of rays of the same colour under certain circumstances. To take a simple example, let two sets of primitive undulations have coincident axes and the same value of λ . Then, according to that law, the velocity at any point of the common axis will be given by the expression

$$m \sin \frac{2\pi}{\lambda} (\kappa at - z + c) + m' \sin \frac{2\pi}{\lambda} (\kappa at - z + c').$$

It will be seen from this expression that if the phases c and c' be the same, or differ by an even multiple of $\frac{\lambda}{2}$, the two sets of undulations are in exact accordance, and the resulting value of the maximum velocity is the sum of m and m' ; but if the difference of phase be an odd multiple of $\frac{\lambda}{2}$, that the undulations are in complete discordance, and the resulting maximum velocity is the difference of m and m' . In the latter case, if $m=m'$, the velocity vanishes at all points of the axis. Also the general values of V , w , and σ show that in the same case the direct and transverse velocities and the condensation vanish at all distances from the axis. Consequently the combination of the undulations under these circumstances would have no effect in producing light. Not only has this theoretical result been verified experimentally in the combination of rays of light, but, as is known, experiment has also indicated the same interference of undulations of the air under like circumstances, at least so far as regards direct vibrations. This fact is confirmatory of the hypothesis that the æther is constituted like the air.

Hitherto we have had under consideration only undulations of that form which, as being independent of arbitrary conditions, they take initially whatever be the mode of their generation, and which for that reason I have called the primitive form. The characteristic of such undulations is the symmetrical arrangement of the direct and transverse velocities and of the condensation about their axes, which is at once explanatory of the term *non-polarized* applied by experimentalists to rays of the form in which they are originally generated. But this symmetry may be disturbed by arbitrary conditions, as is proved by the analytical circumstance that the value of the factor f is determined generally by the integration of the partial differential equation

$$\frac{d^2f}{dx^2} + \frac{d^2f}{dy^2} + 4ef = 0 \text{ (art. 26).}$$

In art. 28 I have obtained a particular solution of this equation, which indicates that the transverse motion is wholly perpendicular to a plane the position of which depends on an arbitrary angle θ contained in the integral. Consequently to produce such transverse motion, it is only required to impress on a primitive ray a disturbance symmetrical with respect to a plane, but in other respects not restricted. Accordingly it is found by experiment that rays submitted to such disturbances are *polarized*, and the plane of symmetry of the disturbance is *the plane of polarization*. It is also shown (arts. 40 and 41), in conformity with experience, that when a primitive ray is so modified, *equal*

portions are polarized in planes at right angles to each other. This resolution, it appears (art. 42), applies only to transverse motions immediately contiguous to the axis,—which, however, is all that the theory requires, reasons having been already given for concluding that the transverse motions of the primitive ray, so far as they are light-producing, are similarly restricted.

For a polarized ray, the transverse vibrations of which are parallel to the plane of xz , f is equal to $1 - \frac{8x^2}{\lambda^2}$ (art. 28), its value for a point on the axis being supposed to be unity. Hence the equations which give the velocities and condensation of a ray completely polarized in the plane of xz are the following:—

$$u = \phi \frac{df}{dx} = \frac{8mx}{\pi\lambda} \cos \frac{2\pi}{\lambda} (\kappa at - z + c),$$

$$w = f \frac{d\phi}{dz} = m \left(1 - \frac{8x^2}{\lambda^2}\right) \sin \frac{2\pi}{\lambda} (\kappa at - z + c),$$

$$\sigma = -\frac{f}{a^2} \cdot \frac{d\phi}{dt} = \frac{m\kappa}{a} \left(1 - \frac{8x^2}{\lambda^2}\right) \sin \frac{2\pi}{\lambda} (\kappa at - z + c);$$

and the transverse accelerative force is

$$-a^2 \frac{d\sigma}{dx} = \frac{16m\kappa ax}{\lambda^2} \sin \frac{2\pi}{\lambda} (\kappa at - z + c).$$

From these equations it is evident that the circumstances under which two rays polarized in the same plane, and having coincident axes and the same value of λ , interfere or coalesce, are precisely the same as those already found for two primitive rays. But if the rays be polarized in planes at right angles to each other, different results are obtained. Let the two rays be in all other respects exactly alike, and, first, let their phases be the same, or differ by an even multiple of $\frac{\lambda}{2}$. Then we have for the transverse velocity (v) in the plane of yz ,

$$v = \phi \frac{df}{dy} = \frac{8my}{\pi\lambda} \cos \frac{2\pi}{\lambda} (\kappa at - z + c).$$

Hence the resulting transverse velocity, or $(u^2 + v^2)^{\frac{1}{2}}$, is

$$\frac{8mr}{\pi\lambda} \cos \frac{2\pi}{\lambda} (\kappa at - z + c);$$

that is, the same as for a primitive ray, the value of m for which is double that for each of the polarized rays. This will also be the case with respect to the resulting values of w , σ , and the transverse accelerative force; so that the compound ray will differ in no respect from a primitive ray. If now the difference of phase

be an odd multiple of $\frac{\lambda}{2}$, the value of $(u^2 + v^2)^{\frac{1}{2}}$ and the resultant of the transverse accelerative forces will plainly be the same as in the former case; but for the resulting values of w and σ we shall have

$$w = \frac{a\sigma}{\kappa} = \frac{8m(y^2 - x^2)}{\lambda^2} \sin \frac{2\pi}{\lambda} (\kappa at - z + c).$$

Hence w and σ will each be very small, on account of the small ratios of y and x to λ . And besides this, the dynamic effect of the undulations in the direction of z must be estimated by the sum of the values of σ within a small circular space about the axis. But clearly within such a space the positive and negative values of the right-hand side of the above equation are equal, and the sum is consequently zero. Thus we may conclude that when the difference of phase is an odd multiple of $\frac{\lambda}{2}$, the direct vibrations are wholly inoperative. But experiment has proved that the light perceived in this case is precisely the same as in the other. Hence it necessarily follows that the perception of light is in no degree affected by the direct vibrations, and that *it is entirely due to transverse vibrations*.

This law being established, we may next conclude that the undulations of two oppositely polarized rays produce independent luminous effects, simply for the reason that their transverse accelerative forces act in rectangular directions. Also since, as is known by experience, the luminous effect of a series of undulations is independent of phase, it follows that the combined luminous effect of two oppositely polarized series is independent of difference of phase. Thus the theory explains the experimental fact, that *oppositely polarized rays having a common path do not interfere, whatever be the difference of their phases*.

It is shown above that the undulations of two oppositely polarized rays into which a primitive ray has been separated produce, when they have a common axis, the same luminous effect on the eye as the undulations of the primitive. Hence, as the two rays are equal, *the intensity of each polarized ray is just half that of the primitive ray*. This law may also be deduced from the principle that *intensity is measured by the square of the velocity of vibration of the æther*. For, on this principle, if the velocity in the primitive ray be called $\phi(r)$, and r make any angle α with the axis of x , the total intensities in the primitive and the polarized rays are respectively measured by

$\iint (\phi(r))^2 r d\alpha dr$, $\iint (\phi(r) \cos \alpha)^2 r d\alpha dr$, $\iint (\phi(r) \sin \alpha)^2 r d\alpha dr$,
the integrations being taken from $\alpha=0$ to $\alpha=2\pi$, and from $r=0$

to $r =$ an arbitrary value; and it is readily seen that the two latter integrals are each equal to half the other.

Having recently found that the resolution of a polarized ray into two parts admits of a much more simple treatment than that referred to in art. 42, I propose to introduce here the discussion of that problem. There are only two conditions which the resolved parts of a polarized ray are required to satisfy, viz. that the sum of their condensations at corresponding points be equal to the condensation at the corresponding point of the integral ray, and that the velocities at corresponding points be the parts, resolved in directions parallel and perpendicular to the new plane of polarization, of the velocity in the integral ray at the same corresponding point. Let that plane make the angle θ with the axis of x , and let s, σ_1, σ_2 be the condensations at any corresponding points of the original and resolved rays, and f, f_1, f_2 be the factors for the same points, the values of which must satisfy the equation (10) in art. 26. Then, if we assume that $\sigma_1 = s \cos^2 \theta$ and $\sigma_2 = s \sin^2 \theta$, we have $\sigma_1 + \sigma_2 = s$, and the first condition is satisfied. Also it will appear from the following considerations that the other condition is satisfied by the same suppositions. Let S, Σ_1, Σ_2 be the condensations at the points of the axes cut by the transverse planes in which the condensations are s, σ_1, σ_2 . Then, from what is shown in arts. 39 and 40,

$$f_1 \Sigma_1 = \sigma_1 = s \cos^2 \theta = f S \cos^2 \theta,$$

$$f_2 \Sigma_2 = \sigma_2 = s \sin^2 \theta = f S \sin^2 \theta.$$

Hence

$$\frac{\Sigma_1 f_1}{S} = f \cos^2 \theta, \quad \frac{\Sigma_2 f_2}{S} = f \sin^2 \theta;$$

also

$$\frac{\phi \Sigma_1}{S} \cdot \frac{df_1}{dx} = \phi \frac{df}{dx} \cos^2 \theta, \quad \frac{\phi \Sigma_2}{S} \cdot \frac{df_2}{dx} = \phi \frac{df}{dx} \sin^2 \theta.$$

The left-hand sides of the last two equations are the parts of the velocities in the bifurcated rays resolved parallel to the axis of x . The other sides of the equations are resolutions of the velocity $\phi \frac{df}{dx}$ of the original ray (which is by supposition wholly parallel to the axis of x) into parts parallel and perpendicular to the new plane of polarization, which parts are then resolved in the direction of the axis of x . Hence the velocities parallel and perpendicular to that plane, which are the velocities of the bifurcated rays, are respectively equal to $\phi \frac{df}{dx} \cos \theta$ and $\phi \frac{df}{dx} \sin \theta$, that is, the parts of the original velocity resolved in those two directions.

Since, therefore, both the required conditions are satisfied by means of the equations $\sigma_1 = s \cos^2 \theta$ and $\sigma_2 = s \sin^2 \theta$, it may be concluded that these equations are true.

From the above results it appears that the intensities of the resolved rays are in the ratio of $\cos^2 \theta$ to $\sin^2 \theta$, and that the sum of their intensities is equal to the intensity of the original ray. If $\theta = 45^\circ$, the two intensities are equal, and we have also

$$\sigma_1 = \sigma_2 = \frac{s}{2}.$$

The two rays of this second polarization, like those of the first, produce independent luminous effects on the eye, because their vibrations are in planes at right angles to each other. Hence, if their phases be different, the total luminous effect will remain the same. The compound ray is not, however, identical in its properties with a ray of common light, the resulting transverse vibrations not being of the same character, as will be seen by the following argument. Let the plane of second polarization be now the plane of xz , and let the transverse velocity due to the vibrations in that plane be $\mu\phi(x) \sin \frac{2\pi}{\lambda} (\kappa at - z + c)$, and that due to the vibrations in the plane of yz be

$$\mu'\phi(y) \sin \frac{2\pi}{\lambda} (\kappa at - z + c').$$

Then, supposing x, y, z to be the coordinates of a *given* particle of the æther at any time t , we have

$$\frac{dx}{dt} = \mu\phi(x) \sin \frac{2\pi}{\lambda} (\kappa at - z + c), \quad \frac{dy}{dt} = \mu'\phi(y) \sin \frac{2\pi}{\lambda} (\kappa at - z + c').$$

In obtaining from these equations the projection of the path of the given particle on the plane of xy , the variations of z may be neglected; and we may also leave out of consideration the changes of x and y in the coefficients $\mu\phi(x)$ and $\mu'\phi(y)$ due to the small changes of position of the particle. By integrating, and eliminating t from the results, an equation between x and y of the following form will be obtained:—

$$\left(\frac{x-a}{A}\right)^2 + \left(\frac{y-b}{B}\right)^2 - 2 \cdot \frac{x-a}{A} \cdot \frac{y-b}{B} \cos \frac{2\pi}{\lambda} (c-c') = \sin^2 \frac{2\pi}{\lambda} (c-c').$$

This equation shows that if $c-c'$ be zero, or any multiple of $\frac{\lambda}{2}$, the left-hand side of the equation is a complete square, and the path of the particle is *a straight line*. It also shows that in general the path is *an ellipse*, and in the particular cases in which $c-c' = \frac{2p+1}{4} \cdot \lambda$ that it is *a circle*. These theoretical

results explain the characters of the different kinds of light which have been named *plane-polarized*, *elliptically-polarized*, and *circularly-polarized*.

Another problem in the theory of light the answer to which depends only on properties of the medium through which it is transmitted, is the law of the variation of its intensity with the distance from a centre of emanation. The investigation of the law of the variation of the velocity and condensation in waves propagated from a centre is given in articles 36 and 37, where, it is to be understood, the waves are resultants of the composition of the ray-undulations whose properties we have been discussing. It appears from that investigation that, at distances from the centre not very small, the resulting velocity and the condensation both vary inversely as the distance from the centre. It is true that the rays were supposed to be so compounded as to destroy transverse vibration, and that the law, being proved only for direct vibrations, would seem not to be established for the case of light. But it should be observed that the action of the eye on a bundle of rays is to bring their axes to a focus, and that this concentration has the effect of making the transverse motions reappear. The dynamical action, on the eye, of the resultant transverse motion of a bundle of rays is, as I have already remarked, of the same kind as that of the transverse motion of a single ray, and the resultant maximum transverse velocity is proportional to the maximum resultant direct velocity, independently of the differences of the values of λ and c in the separate components. Thus the effective transverse velocity will vary inversely as the distance from the centre of emanation; and as the intensity of the luminous effect varies as the square of this velocity, it follows that *the intensity of light emanating from a centre varies inversely as the square of the distance from the centre*.

This law seems also to admit of the following demonstration, which is independent of any special consideration of the action of the undulations on the eye. As the compound undulations from a centre maintain the same composite character during their propagation, the number of axes of individual rays included within a given circular area, as the pupil of the eye, will at different distances from the source of light vary inversely as the square of the distance. But the aggregate luminous effect must, *cæteris paribus*, be proportional to that number. Hence, as before, the intensity varies inversely as the square of the distance.

There is still another class of facts the explanation of which depends exclusively on properties of the æthereal medium, viz. *the effect of compounding colours*, it being assumed that every result of such composition which the eye perceives corresponds

to a result of the composition of undulations. Having treated of this subject expressly in an article on "The Theory of the Composition of Colours," contained in the *Philosophical Magazine* for November 1856, I shall at present simply refer to the arguments there adduced, as giving evidence that the theory of undulations accounts for many phenomena of that class. The subject, however, is open to further investigation both theoretically and experimentally.

I have now completed the consideration of those phenomena of light the theoretical explanations of which depend only on properties of the assumed æthereal medium. The facts and laws which have received explanations have been indicated by italics, in order that their number, variety, and speciality may be the more readily perceived. When it is considered that all these explanations have been evolved by mathematics alone from the single hypothesis that light is generated and transmitted in a fluid medium the pressure of which varies as its density, the evidence for the actual existence of such a medium would seem to be as complete as any to be met with in the whole range of applied mathematics. All phenomena that have been thought to be explained on any other hypothesis are capable of explanation on this, together with a great many more. I cannot, therefore, imagine any reason for giving preference to the theory which attributes the phenomena of light to the oscillations of the discrete atoms of a hypothetically constituted medium, especially as I have also shown, by arguments unanswered and unanswerable, that that theory is beset with insuperable difficulties.

I feel called upon to take this opportunity for adverting again to the reference made by Professor Tyndall to my views on Laplace's theory of the velocity of sound, because, when I wrote the reply at the end of my communication to the *Philosophical Magazine* of last August, I had not had before me the very remarkable and significant experimental results contained in his paper "On the Absorption and Radiation of Heat by Gaseous Matter" (*Phil. Trans.* part 1, 1862, and *Phil. Mag.* for October 1862). These results have a direct bearing on the question at issue, but not such as Professor Tyndall supposes them to have. As far as regards my discussion with Professor Stokes, the principal *physical* point involved was, the sense in which I used the term "radiation," which he said was different from that in which he usually understood it. This might well be the case, because the fact which the term was intended to express was looked at from a theoretical point of view, and did not at that time admit of being substantiated for want of experiments. These have now been supplied by Prof. Tyndall, as a few words will suffice to show. I had long ago conceived that the expla-

nation of the development of heat caused by sudden compression of the air was, the simple fact that the compression brings suddenly fresh atoms into the space through which it extends, and that these instantly become additional centres of secondary heat-undulations, as explained in my Mathematical Theory of Heat contained in the Philosophical Magazine for March 1859. The primary waves from which the secondary are produced, are the heat-bearing waves traversing the space under the ordinary circumstances of *steady temperature*. The new waves destroy for a time the calorific equilibrium, and by propagation from their centres are expended in raising the temperature of all neighbouring bodies more or less distant. This is the kind of radiation I had in view, which, as it seems to me, corresponds exactly to that which on experimental grounds Prof. Tyndall calls "Dynamic Radiation." The surprise felt by the experimenter on witnessing this effect, sufficiently indicates that he had discovered a class of facts not previously known. It is clear that the sudden abstraction of a portion of air would suddenly diminish the number of centres of secondary waves contributing to the maintenance of the calorific equilibrium, and thus would have the opposite effect of absorbing heat and causing a depression of the temperature of surrounding substances. Since the experiments show that for the same change of density dynamic absorption is equal to dynamic radiation, little or no calorific effect can be attributed to the motion through the æther of the introduced or abstracted atoms, as this motion would in both cases tend to raise the temperature. The experiments were so conducted as to determine with precision the different degrees in which the air and different gases contained in the space into which the air flowed were heated by the developed caloric; and it is very much to the present purpose to remark that, of all the gases, not excepting oxygen and hydrogen, the air itself was least heated. Olefiant gas was heated nine times as much. Like results were obtained with respect to the chilling effects. These facts distinctly indicate the small susceptibility of air to acquire, or part with, *temperature*. I readily concede to Prof. Tyndall that his experiments fully establish this law. But in asserting that the experiments decide against my views, he surely could not be aware that the difficulty I have always felt, and expressed as clearly as I could, in regard to Laplace's theory is, that it demands an *extremely great susceptibility* of the air to acquire temperature in the condensed parts of a wave, and to part with temperature in the rarefied parts. Now, even when the free caloric developed by compression is prevented from escaping by confining the compressed air in a closed space, the acquired temperature falls short of the amount which that theory requires, and is, more-

over, not acquired instantly; and it is wholly impossible that when the compression takes place in unconfined air, in which the free caloric has liberty to disperse to any extent, the temperature acquired under the same compression can be the same as in that experiment. For all these reasons I conclude that Prof. Tyndall's experiments are entirely favourable to my views, and that Laplace's theory is inadequate to account for the observed velocity of sound; and as I am entitled to infer, from the long silence mathematicians have maintained on this subject, that the mathematical part of the old theory is untenable, I hope not to have again the trouble of discussing this question.

Cambridge, Nov. 17, 1862.

LXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 406.]

January 23, 1862.—Major-General Sabine, R.A., President, in the Chair.

THE following communications were read:—

“Contributions towards the History of the Monamines.—No. V. Action of Chloracetic Ether on Triethylamine and Triethylphosphine.” By A. W. Hofmann, LL.D., F.R.S.

“Additional Observations and Experiments on the Influence of Physical Agents in the Development of the Tadpole and the Frog.” By John Higginbottom, Esq., F.R.S.

“Note on Internal Radiation.” By George G. Stokes, M.A., Sec. R.S.

In the eleventh volume of the ‘Proceedings of the Royal Society,’ p. 193*, is the abstract of a paper by Mr. Balfour Stewart, in which he deduces an expression for the internal radiation in any direction within a uniaxal crystal from an equation between the radiations incident upon and emerging from a unit of area of a plane surface, having an arbitrary direction, by which the crystal is supposed to be bounded. With reference to this determination he remarks (p. 196), “But the internal radiation, if the law of exchanges be true, is clearly independent of the position of this surface, which is indeed merely employed as an expedient. This is equivalent to saying that the constants which define the position of the bounding surface must ultimately disappear from the expression for the internal radiation.” This anticipation he shows is verified in the case of the expression deduced, according to his principles, for the internal radiation within a uniaxal crystal, on the assumption that the wave-surface† is the sphere and spheroid of Huygens.

* Phil. Mag. S. 4, vol. xxiii. p. 328.

† To prevent possible misapprehension, it may be well to state that I use this term to denote the surface, whatever it may be, which is the locus of the points reached in a given time by a disturbance propagated in all directions from a given point;

In the case of an uncrystallized medium, the following is the equation obtained by Mr. Stewart in the first instance.

Let R, R' be the external and internal radiations in directions OP, OP' , which are connected as being those of an incident and refracted ray, the medium being supposed to be bounded by a plane surface passing through O . Let OP describe an elementary conical circuit enclosing the solid angle $\delta\phi$, and let $\delta\phi'$ be the elementary solid angle enclosed by the circuit described by OP' . Let i, i' be the angles of incidence and refraction. Of a radiation proceeding along PO , let the fraction A be reflected and the rest transmitted; and of a radiation proceeding internally along $P'O$ let the fraction A' be reflected, and the rest transmitted. Then by equating the radiation incident externally on a unit of surface, in the directions of lines lying within the conical circuit described by OP , with the radiation proceeding in a contrary direction, and made up partly of a refracted and partly of an externally reflected radiation, we obtain

$$R \cos i \delta\phi = (1 - A') R' \cos i' \delta\phi' + A R \cos i \delta\phi,$$

$$\text{or} \quad (1 - A) R \cos i \delta\phi = (1 - A') R' \cos i' \delta\phi'. \quad (1)$$

In the case of a crystal there are *two* internal directions of refraction, OP_1, OP_2 , corresponding to a given direction PO of incidence, the rays along OP_1, OP_2 being each polarized in a particular manner. Conversely, there are two directions, P_1O, P_2O , in which a ray may be incident internally so as to furnish a ray refracted along OP , and in each case no second refracted ray will be produced, provided the incident ray be polarized in the same manner as the refracted ray OP_1 or OP_2 . In the case of a crystal, then, equation (1) must be replaced by

$$(1 - A) R \cos i \delta\phi = (1 - A_1) R_1 \cos i_1 \delta\phi_1 + (1 - A_2) R_2 \cos i_2 \delta\phi_2. \quad (2)$$

In the most general case it does not appear in what manner, if at all, equation (2) would split into two equations, involving respectively R_1 and R_2 . For if an incident ray PO were so polarized as to furnish only one refracted ray, say OP_1 , a ray incident along P_1O and polarized in the same manner as OP_1 would furnish indeed only one refracted ray, in the direction OP , but that would be polarized differently from PO ; so that the two systems are mixed up together.

But if the plane of incidence be a principal plane, and if we may assume that such a plane is a plane of symmetry as regards the optical properties of the medium*, the system of rays polarized in and the

I do not use it as a name for the surface defined analytically by the equation

$$(x^2 + y^2 + z^2)(a^2x^2 + b^2y^2 + c^2z^2) - a^2(b^2 + c^2)x^2 - b^2(c^2 + a^2)y^2 - c^2(a^2 + b^2)z^2 + a^2b^2c^2 = 0.$$

As the term *wave-surface* in its physical signification is much wanted in optics, the surface defined by the above equation should, I think, be called *Fresnel's surface*, or *the wave-surface of Fresnel*.

* According to Sir David Brewster (Report of the British Association for 1836, part ii. p. 13, and for 1842, part ii. p. 13), when light is incident on a plane surface of Iceland spar in a plane parallel to the axis, the plane of incidence, which is a principal plane, is not in general a plane of optical, any more than of crystalline symmetry as regards the phenomena of reflexion, although, as is well known,

system polarized perpendicularly to the plane of incidence will be quite independent of each other, and the equality between the radiation incident externally and that proceeding in the contrary direction, and made up partly of a refracted and partly of an externally reflected radiation, must hold good for each system separately. In this case, then, (2) will split into two equations, each of the form (1), R now standing for half the whole radiation, and R' , A' , &c. standing for R_1 , A_1 , &c., or R_2 , A_2 , &c., as the case may be. It need hardly be remarked that the value of A is different in the two cases, and that R' has a value which is no longer, as in the case of an isotropic medium, alike in all directions. In determining according to Mr. Stewart's principles the internal radiation in any given direction within a uniaxal crystal, no limitation is introduced by the restriction of equation (1) to a principal plane, since we are at liberty to imagine the crystal bounded by a plane perpendicular to that containing the direction in question and the axis of the crystal.

Mr. Stewart further reduces equation (1) by remarking that in an isotropic medium, as we have reason to believe, $A' = A$, and that the same law probably holds good in a crystal also, so that the equal factors $1 - A$, $1 - A'$ may be struck out. Arago long ago showed experimentally that light is reflected in the same proportion externally and internally from a plate of glass bounded by parallel surfaces; and the formulæ which Fresnel has given to express, for the case of an isotropic medium, the intensity of reflected light, whether polarized in a plane parallel or perpendicular to the plane of incidence, are consistent with this law. In a paper published in the fourth volume of the Cambridge and Dublin Mathematical Journal (p. 1), I have given a very simple demonstration of Arago's law, based on the sole hypothesis that the forces acting depend only on the positions of the particles. This demonstration, I may here remark, applies without change to the case of a crystal whenever the plane of incidence is a plane of optical symmetry. It may be rendered still more general by supposing that the forces acting depend, not solely on the positions of the particles, but also on any differential coefficients of the coordinates which are of an even order with respect to the time,—a generalization which appears not unimportant, as it is applicable to that view of the mutual relation of the ether and ponderable matter, according to which the ether is compared to a fluid in which a number of solids are immersed, and which in moving as a whole is obliged to undergo local dislocations to make way for the solids.

On striking out the factors $1 - A$ and $1 - A'$, equation (1) is reduced to

$$\frac{R'}{R} = \frac{\cos i \frac{\partial \phi}{\partial i}}{\cos i' \frac{\partial \phi'}{\partial i'}} \cdot \cdot \cdot \cdot \cdot \cdot \cdot \quad (3)$$

all planes passing through the axis are alike as regards internal propagation and the polarization of the refracted rays. Hence, strictly speaking, the statement as to the independence of the two systems of rays should be confined to the case in which the principal plane is also a plane of crystalline symmetry. As, however, the unsymmetrical phenomena were only brought out when the ordinary reflexion was weakened, almost annihilated, by the use of oil of cassia, we may conclude that under common circumstances they would be insensible.

In the case of an isotropic medium, R and R' are alike in all directions, and therefore the ratio of $\cos i \delta\phi$ to $\cos i' \delta\phi'$ ought to be independent of i , as it is very easily proved to be. The same applies to a uniaxal crystal, so far as regards the ordinary ray. But as regards the extraordinary, it is by no means obvious that the ratio should be expressible in the form indicated—as a quantity depending only on the direction OP' . Mr. Stewart has, however, proved that this is the case, independently of any restriction as to the plane of incidence being a principal plane, on the assumption that the wave-surface has the form assigned to it by Huygens.

It might seem at first sight that this verification was fairly adducible in confirmation of the truth of the whole theory, including the assumed form of the wave-surface. But a little consideration will show that such a view cannot be maintained. Huygens's construction links together the law of refraction and the form of the wave-surface, in a manner depending for its validity only on the most fundamental principles of the theory of undulations. The construction which Huygens applied to the ellipsoid is equally applicable to any other surface; it was a mere guess on his part that the extraordinary wave-surface in Iceland spar was an ellipsoid; and although the ellipsoidal form results from the imperfect dynamical theory of Fresnel, it is certain that rigorous dynamical theories lead to different forms of the wave-surface, according to the suppositions made as to the existing state of things. For every such possible form the ratio expressed by the right-hand member of equation (3) ought to come out in the form indicated by the left-hand member, and not to involve explicitly the direction of the refracting plane: and as it seemed evident that it could not be possible, merely by such general considerations as those adduced by Mr. Stewart, to distinguish between those surfaces which were and those which were not dynamically possible forms of the wave-surface, I was led to anticipate that the possibility of expressing the ratio in question under the form indicated was a general property of surfaces. The object of the present Note is to give a demonstration of the truth of this anticipation, and thereby remove from the verification the really irrelevant consideration of a particular form of wave-surface; but it was necessary in the first instance to supply some steps of Mr. Stewart's investigation which are omitted in the published abstract.

The proposition to be proved may be somewhat generalized, in a manner suggested by the consideration of internal reflexion within a crystal, or refraction out of one crystallized medium into another in optical contact with it. Thus generalized it stands as follows:—

Imagine any two surfaces whatsoever, and also a fixed point O ; imagine likewise a plane Π passing through O . Let two points P, P' , situated on the two surfaces respectively, and so related that the tangent planes at those points intersect each other in the plane Π , be called *corresponding points with respect to the plane Π* . Let P describe, on the surface on which it lies, an infinitesimal closed circuit, and P' the “corresponding” circuit; let $\delta\phi, \delta\phi'$ be the solid angles subtended at O by these circuits respectively, and i, i' the inclinations of OP, OP'

to the normal to Π . Then shall the ratio of $\cos i \delta\phi$ to $\cos i' \delta\phi'$ be of the form $[P] : [P']$, where P depends only on the first surface and the position of P , and $[P']$ only on the second surface and the position of P' . Moreover, if either surface be a sphere having its centre at O , the corresponding quantity $[P]$ or $[P']$ shall be constant.

It may be remarked that the two surfaces may be merely two sheets of the same surface, or even two different parts of the same sheet.

Instead of comparing the surfaces directly with each other, it will be sufficient to compare them both with the same third surface; for it is evident that if the points P, P' correspond to the same point P_1 , on the third surface, they will also correspond to each other. For the third surface it will be convenient to take a sphere described round O as centre with an arbitrary radius, which we may take for the unit of length. The letters P_1, i_1, ϕ_1 will be used with reference to the sphere.

Let the surface and sphere be referred to rectangular coordinates, O being the origin, and Π the plane of xy . Let x, y, z be the coordinates of P ; ξ, η, ζ those of P_1 . Then x, y, z will be connected by the equation of the surface, and ξ, η, ζ by the equation

$$\xi^2 + \eta^2 + \zeta^2 = 1.$$

According to the usual notation, let

$$\frac{dz}{dx} = p, \quad \frac{dz}{dy} = q, \quad \frac{d^2z}{dx^2} = r, \quad \frac{d^2z}{dxdy} = s, \quad \frac{d^2z}{dy^2} = t.$$

The equations of the tangent planes at P, P_1, X, Y, Z being the current coordinates, are

$$\begin{aligned} Z - z &= p(X - x) + q(Y - y), \\ \xi X + \eta Y + \zeta Z &= 1; \end{aligned}$$

and those of their traces on the plane of xy are

$$\begin{aligned} pX + qY &= px + qy - z, \\ \xi X + \eta Y &= 1; \end{aligned}$$

and in order that these may represent the same line, we must have

$$\xi = \frac{p}{px + qy - z}, \quad \eta = \frac{q}{px + qy - z}. \quad (4)$$

To the element $dxdy$ of the projection on the plane of xy of a superficial element at P , belongs the superficial element $dS = \sqrt{1 + p^2 + q^2} dxdy$, and to this again belongs the elementary solid angle $\frac{\cos \nu dS}{\rho^2}$, where $\rho = OP$, and ν is the angle between the normal at P and the radius vector. Hence the total solid angle within a small contour is $\frac{\cos \nu}{\rho^2} \sqrt{1 + p^2 + q^2} \iint dxdy$, the double integral being taken within the projection of that small contour. Also $\cos i = \frac{z}{\rho}$. Hence

$$\cos i \delta\phi = \frac{z \cos \nu}{\rho^3} \sqrt{1 + p^2 + q^2} \iint dxdy;$$

and applying this formula to the sphere by replacing $z\sqrt{1+p^2+q^2}$ by 1, ν by 0, and ρ by 1, we have

$$\cos i_1 \delta \phi_1 = \iint d\xi d\eta,$$

the double integral being taken over the projection of the corresponding small area of the sphere.

Now by the well-known formula for the transformation of multiple integrals we have

$$\iint d\xi d\eta = \iint \left(\frac{d\xi}{dx} \frac{d\eta}{dy} - \frac{d\xi}{dy} \frac{d\eta}{dx} \right) dx dy;$$

and therefore

$$\frac{\cos i \delta \phi}{\cos i_1 \delta \phi_1} = \frac{z \cos \nu \sqrt{1+p^2+q^2}}{\rho^3 \left(\frac{d\xi}{dx} \frac{d\eta}{dy} - \frac{d\xi}{dy} \frac{d\eta}{dx} \right)}.$$

But the first of equations (4) gives

$$\begin{aligned} d\xi &= \frac{(px+qy-z)dp - p(xdp+ydq)}{(px+qy-z)^2} \\ &= \frac{\{(qy-z)r - pys\} dx + \{(qy-z)s - pyt\} dy}{(px+qy-z)^2}. \end{aligned}$$

Similarly,

$$d\eta = \frac{\{(px-z)t - qxs\} dy + \{(px-z)s - qxr\} dx}{(px+qy-z)^2}.$$

Hence

$$\frac{d\xi}{dx} \frac{d\eta}{dy} - \frac{d\xi}{dy} \frac{d\eta}{dx} = \frac{V}{(px+qy-z)^4},$$

where

$$\begin{aligned} V &= \{(qy-z)r - pys\} \{(px-z)t - qxs\} - \{(qy-z)s - pyt\} \{(px-z)s - qxr\} \\ &= \{(qy-z)(px-z) - pqxy\} (rt - s^2) \\ &= z(z - px - qy)(rt - s^2). \end{aligned}$$

Hence

$$\frac{\cos i \delta \phi}{\cos i_1 \delta \phi_1} = \frac{z \cos \nu \sqrt{1+p^2+q^2}}{\rho^3} \frac{(z - px - qy)^3}{z(rt - s^2)}.$$

But if ϖ be the perpendicular let fall from O on the tangent plane at P,

$$z - px - qy = \sqrt{1+p^2+q^2} \cdot \varpi,$$

and therefore

$$\frac{\cos i \delta \phi}{\cos i_1 \delta \phi_1} = \frac{\cos \nu \cdot \varpi^3}{\rho^3} \frac{(1+p^2+q^2)^2}{rt - s^2}.$$

But $\varpi = \rho \cos \nu$. Also the quadratic determining the principal radii of curvature at P is

$$(rt - s^2)v^2 + (\&c.)v + (1+p^2+q^2)^2 = 0;$$

the plates be perfectly transparent, we may treat white light as a whole, neglecting as insignificant the chromatic variations of reflecting power. Let ρ be the fraction of the incident light reflected at the first surface of a plate. Then $1-\rho$ may be taken as the intensity of the transmitted light*. Also, since we know that light is reflected in the same proportion externally and internally at the two surfaces of a plate bounded by parallel surfaces, the same expressions ρ and $1-\rho$ will serve to denote the fractions reflected and transmitted at the second surface. We may calculate ρ in accordance with Fresnel's formulæ from the expressions

$$\sin i' = \frac{\sin i}{\mu}, \quad (1)$$

$$\rho = \frac{\sin^2 (i-i')}{\sin^2 (i+i')}, \text{ or } = \frac{\tan^2 (i-i')}{\tan^2 (i+i')}, \quad (2)$$

according as the light is polarized in or perpendicularly to the plane of incidence.

In the case of perfect transparency, we may in imagination make abstraction of the substance of the plates, and state the problem as follows:—There are $2m$ parallel surfaces (m being the number of plates) on which light is incident, and at each of which a given fraction ρ of the light incident upon it is reflected, the remainder being transmitted; it is required to determine the intensity of the light reflected from or transmitted through the system, taking account of the reflexions, infinite in number, which can occur in all possible ways.

This problem, the solution of which is of a simpler form than that of the general case of imperfect transparency, might be solved by a particular method. As, however, the solution is comprised in that of the problem which arises when the light is supposed to be partially absorbed, I shall at once pass on to the latter.

In consequence of absorption, let the intensity of light traversing a plate be reduced in the proportion of 1 to $1-qdx$ in passing over the elementary distance dx within the plate. Let T be the thickness of a plate, and therefore $T \sec i'$ the length of the path of the light within it. Then, putting for shortness

$$e^{-qT \sec i'} = g, \quad (3)$$

1 to g will be the proportion in which the intensity is reduced by absorption in a single transit. The light reflected by a plate will be made up of that which is reflected at the first surface, and that which suffers 1, 3, 5, &c. internal reflexions. If the intensity of the incident light be taken as unity, the intensities of these various portions will be

$$\rho, (1-\rho)^2 \rho g^2, (1-\rho)^2 \rho^3 g^4, \text{ \&c.}$$

and if r be the intensity of the reflected light, we have, by summing

* In order that the intensity may be measured in this simple way, which saves trouble in the problem before us, we must define the intensity of the light transmitted across the first surface to mean what would be the intensity if the light were to emerge again into air across the second surface without suffering loss by absorption, or by reflexion at that surface.

a geometric series,

$$r = \rho + \frac{(1-\rho)^2 \rho g^2}{1-\rho^2 g^2} \cdot \cdot \cdot \cdot \cdot \quad (4)$$

Similarly, if t be the intensity of the transmitted light,

$$t = \frac{(1-\rho)^2 g}{1-\rho^2 g^2} \cdot \cdot \cdot \cdot \cdot \quad (5)$$

and we easily find

$$r = \rho + g\rho t; \quad r + t = 1 - \frac{(1-\rho)(1-g)}{1-\rho g},$$

which is in general less than 1, but becomes equal to 1 in the limiting case of perfect transparency, in which case $g=1$.

The values of μ , i , and q in any case being supposed known, the formulæ (1), (2), (3), (4), (5) determine r and t , which may now therefore be supposed known. The problem therefore is reduced to the following:—There are m parallel plates of which each reflects and transmits given fractions r , t of the light incident upon it: light of intensity unity being incident on the system, it is required to find the intensities of the reflected and refracted light.

Let these be denoted by $\phi(m)$, $\psi(m)$. Consider a system of $m+n$ plates, and imagine these grouped into two systems, of m and n plates respectively. The incident light being represented by unity, the light $\phi(m)$ will be reflected from the first group, and $\psi(m)$ will be transmitted. Of the latter the fraction $\psi(n)$ will be transmitted by the second group, and $\phi(n)$ reflected. Of the latter the fraction $\psi(m)$ will be transmitted by the first group, and $\phi(m)$ reflected, and so on. Hence we get for the light reflected by the whole system,

$$\phi(m) + (\psi m)^2 \phi(n) + (\psi m)^2 \phi(m) (\phi n)^2 + \dots,$$

and for the light transmitted,

$$\psi(m)\psi(n) + \psi(m)\phi(n)\phi(m)\psi(n) + \psi(m)(\phi n)^2(\phi m)^2\psi(n) + \dots,$$

which gives, by summing the two geometric series,

$$\phi(m+n) = \phi(m) + \frac{(\psi m)^2 \phi(n)}{1 - \phi(m)\phi(n)}; \quad \cdot \cdot \cdot \cdot \cdot \quad (6)$$

$$\psi(m+n) = \frac{\psi(m)\psi(n)}{1 - \phi(m)\phi(n)} \cdot \cdot \cdot \cdot \cdot \quad (7)$$

We get from (6)

$$\phi(m+n)\{1 - \phi(m)\phi(n)\} = \phi(m) + \phi(n)\{(\psi m)^2 - (\phi m)^2\};$$

and the first member of this equation being symmetrical with respect to m and n , we get, by interchanging m and n and equating the results,

$$\phi(m) + \phi(n)\{(\psi m)^2 - (\phi m)^2\} = \phi(n) + \phi(m)\{(\psi n)^2 - (\phi n)^2\};$$

or

$$\frac{1}{\phi(m)}\{1 + (\phi m)^2 - (\psi m)^2\} = \frac{1}{\phi(n)}\{1 + (\phi n)^2 - (\psi n)^2\},$$

which is therefore constant. Denoting this constant for convenience

by $2 \cos \alpha$, we have

$$(\psi m)^2 = 1 - 2 \cos \alpha \cdot \phi(m) + (\phi m)^2. \quad (8)$$

Squaring (7), and eliminating the function ψ by means of (8), we find

$$\begin{aligned} & \{1 - \phi(m)\phi(n)\}^2 \{1 - 2 \cos \alpha \cdot \phi(m+n) + [\phi(m+n)]^2\} \\ &= \{1 - 2 \cos \alpha \cdot \phi(m) + (\phi m)^2\} \{1 - 2 \cos \alpha \cdot \phi(n) + (\phi n)^2\}. \end{aligned} \quad (9)$$

From the nature of the problem, m and n are positive integers, and it is only in that case that the functions ϕ , ψ , as hitherto defined, have any meaning. We may, however, contemplate functions ϕ , ψ of a continuously changing variable, which are defined by the equations (6) and (7); and it is evident that if we can find such functions, they will in the particular case of a positive integral value of the variable be the functions which we are seeking.

In order that equations (6), (7) may hold good for a value zero of one of the variables, suppose n , we must have $\phi(0)=0$, $\psi(0)=1$. The former of these equations reduces (9) for $n=0$ to an identical equation. Differentiating (9) with respect to n , and after differentiation putting $n=0$, we find

$$\begin{aligned} & \phi'(0)\phi(m)\{1 - 2 \cos \alpha \cdot \phi(m) + (\phi m)^2\} + \cos \alpha \cdot \phi'(m) - \phi(m)\phi'(m) \\ &= \cos \alpha \cdot \phi'(0) \{1 - 2 \cos \alpha \cdot \phi(m) + (\phi m)^2\}, \end{aligned}$$

or dividing out by $\phi(m) - \cos \alpha$, (for $\phi(m) = \cos \alpha$ would only lead to $\phi(m) = \cos \alpha = 0$, $\psi(m) = C^m$),

$$\phi'(m) = \phi'(0) \{1 - 2 \cos \alpha \phi(m) + (\phi m)^2\}. \quad (10)$$

Integrating this equation, determining the arbitrary constant by the condition that $\phi(m)=0$ when $m=0$, and writing β for $\sin \alpha \cdot \phi'(0)$, we have

$$\phi(m) = \frac{\sin m\beta}{\sin(\alpha + m\beta)}. \quad (11)$$

Substituting in (8) and reducing, we find

$$(\psi m)^2 = \frac{\sin^2 \alpha}{\sin^2(\alpha + m\beta)}. \quad (12)$$

But (8) was derived, not from (7) directly, but from (7) squared; and on extracting the square root of both sides of (12), we must choose that sign which shall satisfy (7), and therefore we must take the sign +, as we see at once on putting $m=n=0$. The equation (12) on taking the proper root and (11) may be put under the form

$$\frac{\phi(m)}{\sin(m\beta)} = \frac{\psi(m)}{\sin \alpha} = \frac{1}{\sin(\alpha + m\beta)}; \quad (13)$$

and to determine the arbitrary constants α , β we have, putting $m=1$, and $\phi(m)=r$, $\psi(m)=t$,

$$\frac{r}{\sin \beta} = \frac{t}{\sin \alpha} = \frac{1}{\sin(\alpha + \beta)}. \quad (14)$$

We readily get from equations (13),

$$1 - \phi(m)\phi(n) = \frac{\sin \alpha \sin \{\alpha + (m+n)\beta\}}{\sin(\alpha + m\beta) \sin(\alpha + n\beta)};$$

$$\phi(m+n) - \phi(m) = \frac{\sin \alpha \sin n\beta}{\sin \{\alpha + (m+n)\beta\} \sin(\alpha + m\beta)};$$

whence the equations (6), (7) are easily verified. This verification seems necessary in logical strictness, because we have no right to assume *à priori* that it is possible to satisfy (6) and (7) for general values of the variables; and in deriving the equation (10), the equations (6) and (7) were only assumed to hold good for general values of m and infinitely small values of n .

The equations (13), (14) give the following *quasi*-geometrical construction for solving the problem:—Construct a triangle of which the sides represent in magnitude the intensity of the incident, reflected, and refracted light in the case of a single plate, and then, leaving the first side and the angle opposite to the third unchanged, multiply the angle opposite to the second by the number of plates; the sides of the new triangle will represent the corresponding intensities in the case of the system of plates. I say *quasi*-geometrical, because the construction cannot actually be effected, inasmuch as the first side of our triangle is greater than the sum of the two others, and the angles are imaginary.

To adapt the formulæ (13), (14) to numerical calculation, it will be convenient to get rid of the imaginary quantities. Putting

$$\sqrt{\{(1+r+t)(1+r-t)(1+t-r)(1-r-t)\}} = \Delta, \quad (15)$$

we have by the common formulæ of trigonometry,

$$\cos \alpha = \frac{1+r^2-t^2}{2r}; \quad \sin \alpha = \frac{\pm \sqrt{-1}\Delta}{2r};$$

whence, putting

$$\frac{1}{2r}(1+r^2-t^2+\Delta) = a, \quad . \quad . \quad . \quad . \quad (16)$$

we have

$$e^{\sqrt{-1}\alpha} = \cos \alpha + \sqrt{-1} \sin \alpha = a \mp 1.$$

It is a matter of indifference which sign be taken: choosing the under signs, we have

$$2r \sin \alpha = -\sqrt{-1}\Delta, \quad e^{\sqrt{-1}\alpha} = a.$$

We have also

$$\cos \beta = \frac{1+t^2-r^2}{2t}, \quad \sin \beta = \frac{r}{t} \sin \alpha = -\frac{\sqrt{-1}\Delta}{2t},$$

no fresh ambiguity of sign being introduced. Putting therefore

$$\frac{1}{2t}(1+t^2-r^2+\Delta) = b, \quad . \quad . \quad . \quad . \quad (17)$$

we have

$$e^{\sqrt{-1}\beta} = b;$$

and equations (13) now give

$$\frac{\phi(m)}{b^m - b^{-m}} = \frac{\psi(m)}{a - a^{-1}} = \frac{1}{ab^m - a^{-1}b^{-m}} \dots \dots (18)$$

In the case of perfect transparency these expressions take a simpler form. If $r+t$ differ indefinitely little from 1, α and β will be indefinitely small. Making α and β indefinitely small in (13) and (14), and putting $1-r$ for t , we find

$$\frac{\phi(m)}{mr} = \frac{\psi(m)}{1-r} = \frac{1}{1+(m-1)r} \dots \dots (19)$$

In this case it is evident that each of the $2m$ reflecting surfaces might be regarded as a separate plate reflecting light in the proportion of ρ to 1, and therefore we ought also to have, writing $2m$ for m and ρ for r in the denominators of the equations (19),

$$\frac{\phi(m)}{2m\rho} = \frac{\psi(m)}{1-\rho} = \frac{1}{1+(2m-1)\rho} \dots \dots (20)$$

It is easy to verify that when $g=1$ (4) reduces (19) to (20).

The following Table gives the intensity of the light reflected from or transmitted through a pile of m plates for the values 1, 2, 4, 8, 16, 32, and ∞ of m , for three degrees of transparency, and for certain selected angles of incidence. The assumed refractive index μ is 1.52. $\delta=1-e^{-\tau T}$ is the loss by absorption in a single transit of a plate at a perpendicular incidence, so that $\delta=0$ corresponds to perfect transparency. The most interesting angles of incidence to select appeared to be zero and the polarizing angle $\varpi=\tan^{-1} \mu$; but in the case of perfect transparency the result has also been calculated for an angle of incidence a little (2°) greater than the polarizing angle. ϕ denotes the intensity of the reflected and ψ that of the transmitted light, the intensity of the incident light being taken at 1000. For oblique incidences it was necessary to distinguish between light polarized in and light polarized perpendicularly to the plane of incidence; the suffixes 1, 2 refer to these two kinds respectively. For oblique incidences a column is added giving the ratio of ψ_1 to ψ_2 , which may be taken as a measure of the defect of polarization of the transmitted light. No such column was required for $\delta=0$ and $i=\varpi$, because in this case $\psi_2=1000$.

* From a paper by M. Wild in Poggendorff's 'Annalen' [vol. ix. (1856) p. 240] I find that the formulæ for the particular case of perfect transparency have already been given by M. Neumann. His demonstration does not appear to have been published.

<i>m</i>	$\delta=0$						$\delta=\frac{1}{20}$						$\delta=\frac{1}{10}$					
	<i>i</i> =0		<i>i</i> = ϖ		<i>i</i> = $\varpi+2^{\circ}$		<i>i</i> =0		<i>i</i> = ϖ		$\frac{\psi_1}{\psi_2}$		<i>i</i> =0		<i>i</i> = ϖ		$\frac{\psi_1}{\psi_2}$	
	ϕ	ψ	ϕ_1	ψ_1	ϕ_2	ψ_2	ϕ	ψ	ϕ_1	ψ_1	ψ_2	$\frac{\psi_1}{\psi_2}$	ϕ	ψ	ϕ_1	ψ_1	ψ_2	$\frac{\psi_1}{\psi_2}$
1	82	918	271	729	1	999	300	700	1	999	.701		74	826	245	639	881	.725
2	151	849	426	574	2	998	459	541	2	998	.542		125	686	351	435	777	.559
4	262	738	598	402	4	996	628	372	4	996	.373		185	479	427	215	604	.357
8	416	584	749	251	8	992	771	229	8	992	.231		229	237	451	57	365	.156
16	587	413	856	144	16	984	870	130	16	984	.132		243	59	453	4	133	.030
32	740	260	922	78	32	968	931	69	32	968	.071		244	4	453	0	18	.001
∞	1000	0	1000	0	1000	0	1000	0	1000	0	.000		244	0	453	0	0	.000

The intensity of the light reflected from an infinite number of plates, as we see from (18), is a^{-1} ; and since a is changed into a^{-1} by changing the sign of α or of Δ ,

$$a^{-1} = \frac{1}{2r}(1 + r^2 - t^2 - \Delta), \quad . \quad . \quad . \quad . \quad . \quad (21)$$

which is equal to 1 in the case of perfect transparency. Accordingly a substance which is at the same time finely divided, so as to present numerous reflecting surfaces, and which is of such a nature as to be transparent in mass, is brilliantly white by reflected light,—for example snow, and colourless substances thrown down as precipitates in chemical processes.

The intensity of the light reflected from a pile consisting of an infinite number of similar plates falls off rapidly with the transparency of the material of which the plates are composed, especially at small incidence. Thus at a perpendicular incidence we see from the above Table that the reflected light is reduced to little more than one half when 2 per cent. is absorbed in a single transit, and to less than a quarter when 10 per cent. is absorbed.

With imperfectly transparent plates, little is gained by multiplying the plates beyond a very limited number, if the object be to obtain light, as bright as may be, polarized by reflexion. Thus the Table shows that 4 plates of the less defective kind reflect 79 per cent., and 4 plates of the more defective as much as 94 per cent., of the light that could be reflected by a greater number, whereas 4 plates of the perfectly transparent kind reflect only 60 per cent.

The Table shows that while the amount of light transmitted at the polarizing angle by a pile of a considerable number of plates is materially reduced by a defect of transparency, its state of polarization is somewhat improved. This result might be seen without calculation. For while no part of the transmitted light which is polarized perpendicularly to the plane of incidence underwent reflexion, a large part of the transmitted light polarized the other way was reflected an even number of times; and since the length of path of the light within the absorbing medium is necessarily increased by reflexion, it follows that a defect of transparency must operate more powerfully in reducing the intensity of light polarized in, than of light polarized perpendicularly to the plane of polarization. But the Table also shows that a far better result can be obtained, as to the perfection of the polarization of the transmitted light, without any greater loss of illumination, by employing a larger number of plates of a more transparent kind.

Let us now confine our attention to perfectly transparent plates, and consider the manner in which the degree of polarization of the transmitted light varies with the angle of incidence.

The degree of polarization is expressed by the ratio of ψ_1 to ψ_2 , which for brevity will be denoted by χ . When $\chi=1$ there is no polarization; when $\chi=0$ the polarization is perfect, in a plane perpendicular to the plane of incidence. Now ψ (which is used to denote ψ_1 or ψ_2 as the case may be) is given in terms of ρ by one of the equations (20), and ρ is given in terms of $i-i'$ and $i+i'$ by

Fresnel's formulæ (2). Put

$$i - i' = \theta, \quad i + i' = \sigma;$$

then, from (1),

$$\frac{di}{\tan i} = \frac{di'}{\tan i'} = \frac{d\theta}{\tan i - \tan i'} = \frac{d\sigma}{\tan i + \tan i'} = \cos i \cos i' d\omega, \text{ suppose,}$$

whence

$$d\theta = \sin \theta d\omega, \quad d\sigma = \sin \sigma d\omega; \quad . \quad . \quad . \quad (22)$$

and we see that i and ω increase together from $i=0$ to $i=\frac{\pi}{2}$. We have also

$$\rho_1 = \frac{\sin^2 \theta}{\sin^2 \sigma}, \quad d\rho_1 = \frac{2 \sin \theta}{\sin^3 \sigma} (\sin \sigma \cos \theta d\theta - \sin \theta \cos \sigma d\sigma) = \frac{2 \sin^2 \theta}{\sin^2 \sigma} (\cos \theta - \cos \sigma) d\omega$$

$$\rho_2 = \frac{\tan^2 \theta}{\tan^2 \sigma}, \quad d\rho_2 = \frac{2 \tan \theta}{\tan^3 \sigma} (\tan \sigma \sec^2 \theta d\theta - \tan \theta \sec^2 \sigma d\sigma)$$

$$= \frac{2 \sin^3 \theta \cos \sigma}{\cos^3 \theta \sin^2 \sigma} (\cos \sigma - \cos \theta) d\omega = -\frac{\cos \sigma}{\cos^3 \theta} d\rho_1.$$

Now $\cos \theta - \cos \sigma$ or $2 \sin i \sin i'$ is positive; and $\cos \sigma$ is positive from $i=0$ to $i=\varpi$, and negative from $i=$ to $i=\frac{\pi}{2}$. But (20) shows that ψ decreases as ρ increases. From $i=0$ to $i=\varpi$, ρ_1 increases and ρ_2 decreases, and therefore ψ_1 decreases and ψ_2 increases, and therefore on both accounts χ decreases. When $i=\varpi$, $\frac{d\rho_1}{di}$ is still positive, and therefore $\frac{d\psi_1}{di}$ negative, but ψ_2 has its maximum value 1, so that on

passing through the polarizing angle χ still decreases, or the polarization improves. When the plates are very numerous, $\psi_2=1$ at the polarizing angle, and on both sides of it decreases rapidly, whereas ψ_1 , which is always small, suffers no particular change about the polarizing angle. Hence in this case χ must be a minimum a little beyond the polarizing angle. Let us then seek the angle of incidence which makes χ a minimum in the case of an arbitrary number of plates.

We have from (20) and (2),

$$\begin{aligned} \chi &= \frac{\sin^2 \sigma - \sin^2 \theta}{\sin^2 \sigma + (2m-1) \sin^2 \theta} \cdot \frac{\sin^2 \sigma \cos^2 \theta + (2m-1) \sin^2 \theta \cos^2 \sigma}{\sin^2 \sigma \cos^2 \theta - \sin^2 \theta \cos^2 \sigma} \\ &= \frac{\sin^2 \sigma \cos^2 \theta + (2m-1) \sin^2 \theta \cos^2 \sigma}{\sin^2 \sigma + (2m-1) \sin^2 \theta} = 1 - \frac{2m}{\operatorname{cosec}^2 \theta + (2m-1) \operatorname{cosec}^2 \sigma}. \end{aligned} \quad (23)$$

Hence χ is a minimum along with $\operatorname{cosec}^2 \theta + (2m-1) \operatorname{cosec}^2 \sigma$. Differentiating, and taking account of the formulæ (22), we find, to deter-

mine the angle of maximum polarization, the very simple equation

$$\cos \theta \sin^2 \sigma + (2m-1) \cos \sigma \sin^2 \theta = 0. \quad . \quad . \quad (24)$$

For any assumed value of i from ϖ to $\frac{\pi}{2}$, this equation gives at once the value of m , that is, the number of plates of which a pile must be composed in order that the assumed incidence may be that of maximum polarization of the transmitted light. The equation may be put under the form

$$2m-1 = -\frac{\tan \sigma}{\tan \theta} \cdot \frac{\sin \sigma}{\sin \theta} = \frac{1}{\sqrt{\rho_1 \rho_2}}.$$

Now we have seen that both ρ_1 and ρ_2 continually increase, and therefore m continually decreases, from $i=\varpi$ to $i=\frac{\pi}{2}$. At the first of these limits $\rho_2=0$, and therefore $m=\infty$. At the second $\rho_1=\rho_2=1$, and therefore $m=1$. Hence with a single plate the polarization of the transmitted light continually improves up to a grazing incidence, but with a pile of plates the polarization attains a maximum at an angle of incidence which approaches indefinitely to the polarizing angle as the number of plates is indefinitely increased.

Eliminating m from (23) and (24), we find

$$\chi = -\cos \theta \cos \sigma, \quad . \quad . \quad . \quad (25)$$

which determines for any pile χ_1 , the defect of maximum polarization of the transmitted light, in terms of the angle of incidence for which the polarization is a maximum. We have, from (25), (22), and (24),

$d\chi_1 = (\sin^2 \theta \cos \sigma + \sin^2 \sigma \cos \theta) d\omega = -2(m-1) \cos \sigma \sin^2 \theta d\omega$,
 and $\cos \sigma$ is negative. Hence χ_1 decreases as ω (and therefore i) decreases, or as m increases. For $m=1$, $i=\frac{\pi}{2}$ and $\chi_1=\mu^{-2}$; for $m=\infty$, $\cos \sigma=0$, and therefore $\chi_1=0$, or the maximum polarization tends indefinitely to become perfect as the number of plates is indefinitely increased.

For a given number of plates the angle of maximum polarization may be readily found from (24) by the method of trial and error. But for merely examining the progress of the functions, instead of tabulating i for assumed values of m , it will serve equally well to tabulate m for assumed values of i . The following Table gives for assumed angles of incidence, decreasing by 5° from 90° , the number of plates required to make these angles the angles of maximum polarization of the transmitted light, and the value of χ_1 , which determines the defect of polarization.

$i =$	90°	85°	80°	75°	70°	65°	60°	$56^\circ 40' (= \varpi)$
$m =$	1	1.330	1.944	2.913	4.921	9.775	30.372	∞
$\chi_1 =$.433	.422	.390	.337	.265	.177	.075	0

Jan. 30.—“On the Calculus of Symbols.”—Second Memoir. By W. H. L. Russell, Esq., A.B.

“On Internal and External Division in the Calculus of Symbols.” By William Spottiswoode, Esq., M.A., F.R.S.

Phil. Mag. S. 4. Vol. 24. No. 163. Dec. 1862. 2 K

“On the Absorption and Radiation of Heat by Gaseous Matter.”—Second Memoir. By John Tyndall, Esq., F.R.S. (See *Phil. Mag.* for October.)

February 6.—Major-General Sabine, R.A., President, in the Chair.

“On the Motions of Camphor on the Surface of Water.” By Charles Tomlinson, Esq.

The object of this paper is to show that the phenomenon in question is a much more general one than is commonly supposed; that the explanations hitherto given of it have been insufficient or erroneous. The author endeavours to explain the real nature of the phenomenon in a series of experiments and observations, and to establish the following propositions:—

I. That the camphors, or stearoptens of the volatile oils, present phenomena of rotation and progression when thrown on the surface of clean water in a chemically clean vessel.

II. That these phenomena belong also to certain salts, and to a variety of vegetable and other substances containing a liquid that diffuses readily over the surface of water.

III. That solutions of camphor in benzole, in some of the essential oils, &c., present phenomena of rotation and progression on the surface of water—a property which also belongs to creosote, and to some other liquids that do not contain camphor.

IV. That the motions of camphor may be imitated by placing on water miniature rafts or coracles of inert substances, such as talc, tinfoil, paper, &c., smeared with or containing the elæoptens of volatile oils, or indeed any volatile liquid, such as ether, alcohol, chloroform, &c., provided there be some communication and adhesion between such liquid and the surface of the water.

V. That the camphors, &c., being slightly soluble in water, that is, the adhesion of the water partly overcoming the cohesion of the camphor, a film of camphor is thus detached from it, and spread over the surface of the water the moment that the camphor comes in contact therewith.

VI. That the dimensions and form of this film depend on those of the piece of camphor operated on; and, in general, the film separates more easily from broken surfaces and angles of the fragment than from a smooth natural surface, just as the crushed or broken surface of a crystal is more soluble than a perfect crystal.

VII. That such films being constantly detached from the camphor so long as it is in contact with the water, displace each other; the preceding film being conveyed away by the adhesion of the water in radial lines, these produce motion, by reaction on the fragment, causing it to rotate after the manner of a Barker's mill.

VIII. That these radial lines or jets being of unequal intensity, the direction and intensity of the motion will follow that of their resultant.

IX. That the jets or films of camphor can be rendered sensible by various means—as by fixing the camphor partly submerged in water, and dusting the surface lightly with lycopodium powder: a series of horizontal currents produced by the films will then be made visible, which films or jets cause the camphor, when free to move, to rotate on a vertical axis.

X. That the motions of the fragments of camphor on water are greatly influenced and complicated by their mutual attraction and by the attraction of the sides of the vessel.

XI. That the film of camphor diffused over the surface of the water is very volatile, disappearing as fast as it is formed, chiefly into the air, only a very small portion being retained by the water. Hence camphor wastes away much more quickly at the surface of the water than in water alone or in air alone, because at the surface the film is being constantly formed at the expense of the camphor, and is spread out to the united action of air and water.

XII. That whatever interferes with evaporation lowers or arrests the motions of the camphor and the allied phenomena; so, on the contrary, whatever promotes evaporation exalts these phenomena. Effects which are displayed with great energy on a bright and sunny day, are produced either sluggishly or not at all on a wet, dull, or foggy one.

XIII. That a fixed oil forming a film on water will displace the camphor film, and so permanently arrest the motions of the camphor; but a volatile oil will only arrest the motions while it is present and undergoing evaporation.

XIV. That the presence of the camphor film on water will, in some cases, prevent the formation of other films, the liquids that would otherwise form them remaining lenticular.

XV. That the camphor film, and other films, in many cases repel each other on the surface of water.

XVI. That the motions of camphor on the surface of water are accelerated by the action of the vapour of benzole, and some other volatile substances: such vapours, condensing in the liquid form on the camphor, and then being diffused by the adhesion of the water, react on the camphor.

GEOLOGICAL SOCIETY.

[Continued from p. 326.]

November 5, 1862.—Prof. A. C. Ramsay, President, in the Chair.

The following communications were read:—

1. “Descriptions of some Fossils from India, discovered by Dr. Fleming of Edinburgh.” By Dr. L. de Koninck, For. Mem. G.S.

The author gave a detailed description of 44 species of fossils from the western end of the Salt-range of the Punjaub, on the right bank of the Indus, discovered by Dr. J. Fleming and Mr. W. Purdon. The fossils belonged to the following classes:—*Pisces*, *Cephalopoda*, *Lamellibranchiata*, *Bryozoa*, *Echinodermata*, and *Anthozoa*, the *Brachiopoda* having been previously described by Mr. Davidson (Quart. Journ. Geol. Soc. vol. xviii. p. 25). The same mixture of Mesozoic with Palæozoic types observed by that gentleman in the *Brachiopoda* was also noticed by the author in these fossils. He therefore suggests the possibility of a further examination of the strata showing the existence of two intimately associated formations, belonging respectively to the Carboniferous and Lower Mesozoic periods.

2. "On a Deposit containing *Diatomaceæ*, Leaves, &c., in the Iron-ore Mines near Ulverston." By Miss E. Hodgson.

The object of this paper was to show that this deposit, which was first described by Mr. Bolton in the Society's Journal, vol. xviii. p. 274, and considered by him to be of lacustrine origin, was deposited in a large cavern or chain of caverns by a subterranean stream, originating probably in a brook called the 'Poaka Beck.' The authoress first described in detail some of the various caverns and swallow-holes which abound in the limestone of the district; and then alluded to the current belief of their communication with each other, and with springs. Miss Hodgson also remarked that, prior to the year 1842, the Poaka Beck, after having become partially engulfed at Inman Gill, is said to have taken a subterranean course. Since the above-named date, its course has been diverted. Mr. Bolton's sections were then critically examined; and the paper concluded with a list of the *Diatomaceæ* found in the deposit, with notes on the places where they occur in the streams of the district, and with some remarks on the vegetable remains.

3. "On the Geology of a part of the Masulipatam District." By Capt. F. Applegath, Madras Army.

The sections in the country west of the Palair River and north of the Kistna River, described by the author, show the existence of a synclinal in which the following rocks occur, beginning westward and ascending:—Dark-coloured Schist, Red Schist, Black Limestone, and Variegated Schist, the last forming the uppermost bed; from thence descending and proceeding eastward:—Limestone, Micaceous Schists, and Olive and Brown Schists. The last bed lies unconformably upon Flagstone, which is succeeded by coarse Sandstone, and Gneiss forms the basement bed.

4. "On the Association of Granite with the Tertiary Strata near Kingston." By J. G. Sawkins, Esq., F.G.S. In a letter to Sir R. I. Murchison, F.G.S. &c.

The occasion of this letter was the discovery by the author of a granitic formation traversing Jamaica in a direction from S.E. to N.W., being the same as that of the earthquake-shocks. It pierces the Carbonaceous series, and also the Tertiary strata, whence the author concludes that it is of Tertiary age. It usually contains copper-ores, and is often more or less decomposed.

LXV. *Intelligence and Miscellaneous Articles.*

ON THE MODIFICATIONS WHICH THE VELOCITY OF LIGHT EXPERIENCES IN GLASS AND MANY OTHER BODIES UNDER THE INFLUENCE OF HEAT. BY M. H. FIZEAU.

THE following is an analysis of the more complete memoir which is shortly to be published in the *Annales de Chimie et de Physique*.

In the first part several experiments are adduced relative to interference-phenomena, observed with the yellow light emitted by a monochromatic lamp. Making the experiment of Newton's rings

with this light, the two surfaces between which the interferences are produced could be removed to a distance of 15 millimetres, and still the existence of distinctly visible rings was confirmed. The difference between the path of the rays was then about 50,000 undulations. Similar observations could be made with plates of glass whose faces are virtually parallel; and by observing in a perpendicular direction the light reflected from their surfaces, very beautiful interference-fringes could be seen even when the thickness of the glass amounted to 10 millimetres.

Under these circumstances the temperature is seen to exert a very marked influence on the position of the fringes; so that, heating the glass a certain number of degrees, the fringes on the surface are displaced a quantity proportional to the excess of temperature. This displacement, which may be accurately measured, may be referred to two very distinct actions of heat: first, to expansion, which increases the thickness of the glass and consequently the difference in path of the two rays; secondly, to a modification in the velocity of light, and hence in the refractive index. But, knowing the coefficient of the expansion of glass and the temperature, we can calculate by how much the fringes ought to be displaced by this cause alone, and then compare the result of calculation with that of observation.

Thus there are all the elements necessary to decide if the refractive index is modified by heat, and to estimate numerically the modifications which may be produced.

This furnishes a method which may without difficulty be applied to all transparent solid bodies capable of being cut in plates with parallel faces.

The second part of the memoir contains the results of several series of experiments made with substances of different kinds—crown-glass, ordinary flint-glass, a denser variety of flint-glass, fluor-spar, and Iceland spar.

Thus, it has been observed that the refractive index of ordinary glass increases a little, but very slowly, as the temperature rises—a result which agrees with the previous results of Arago and of M. Neumann. With crown-glass the index did not undergo any apparent change.

With flint glass there is a very distinct increase of the index; and with heavy flint the increase is still greater.

Fluor-spar has the singular property that its refractive index diminishes in a marked degree as the temperature rises; it is the only solid body, among those hitherto examined, which behaves in this way. This property is common to it, with all liquids and gases.

Lastly, Iceland spar presented interesting phenomena in reference to the remarkable changes which heat occasions in the form of its crystals and in the intensity of its double refraction, according to the researches of Mitscherlich. The effects produced by heat on the two refractive indices have been capable of being measured, and certain consequences relative to the singular phenomena of expansion of this crystal have been drawn from them.—*Comptes Rendus*, June 23, 1862.

ON MAGNETIZING STEEL NEEDLES BY THE CURRENT OF A LEYDEN JAR. BY DR. PAALZOW.

From the fact that a steel needle, by the discharge of a Leyden jar, may be abnormally magnetized, that is to say in a direction contrary to that required by Ampère's rule, Savary and Helmholtz concluded that there must be an alternating motion of the electricity in the circuit of a Leyden jar. I imagined I had found in the phenomena of Geissler's tubes a means by which it could be decided whether the discharge was simple or alternating; with its help it could be shown whether the occurrence of abnormal magnetism actually depended on a change in the direction of the current.

The proof could be given by magnetizing with currents which, according to the indications of the tubes, had a single direction. Under these circumstances only a normal magnetism should be observed; and when abnormal magnetism occurred, the indications of the tube ought to be those of alternating discharge.

I accordingly induced Dr. von Liphard to investigate the magnetizing of steel needles by the current of the battery. He found that, so long as the Geissler's tubes indicated with certainty a single discharge, abnormal magnetism was never observed; and that whenever abnormal magnetism occurred, the luminous phenomena indicated alternating discharges.

But the proof that abnormal magnetism is to be ascribed to currents in opposite directions may be drawn from the needles themselves. Wiedemann, in his investigations on the magnetization of steel bars (Poggendorff's *Annalen*, vol. c. p. 241), has given a method for deciding whether a magnetic or unmagnetic needle was previously magnetic or had a different magnetism. It appeared to me possible to show by the aid of these means that the abnormally magnetized needles had been previously normally magnetized by the same discharge of a battery, and that in fact a demagnetization had actually taken place.

In common with Prof. Wiedemann I commenced these investigations, and have since continued them. Of the modes of testing, that was chosen which is described in the above treatise in the following words:—

“If the magnetism in a steel bar had been partially or totally withdrawn, or even completely reversed by a current opposite in direction to the original magnetizing current, it regained, on being struck, a part of its first magnetism. Hence it is possible in this way to prepare an unmagnetic bar, which by agitation becomes magnetic. In this case the bar can of course be placed at right angles to the magnetic meridian, and thus withdrawn from the influence of the terrestrial magnetism.”

In these experiments it was shown that perfectly unmagnetic needles, which also remained unmagnetic when they were struck before being magnetized, assumed normal magnetism when struck with unmagnetic substances, after they had been abnormally magnetized by the discharge of a Leyden battery. In these experiments the circuit consisted of short good conducting wires, between which a Riess's air thermometer and a short magnetizing spiral of 27 turns were coiled.

From Wiedemann's experiments, needles in which the magnetism could be increased or reversed by striking must have been previously magnetized in the opposite direction; only one discharge of the battery had magnetized the needles, and therefore these discharges must have taken place in opposite directions. Hence it appears to me justifiable to regard the occurrence of abnormal magnetism as a sign of the alternating direction of the discharge of the Leyden jar.—*Monatsbericht der Akademie zu Berlin*, February 1862.

NOTE ON THE LIMIT OF THE CHARGE OF CONDENSERS.

BY M. GAUGAIN.

The charge of any given condenser placed in connexion with the ground depends on the greater or less time during which the action of the source is continued; at least this is the case when the dielectric is a solid body. Hence in the case of solid dielectrics, the expression representing the quantity of electricity accumulated on the armatures of a condenser ought to be a function of the time during which the condenser is placed in connexion with the source. But although, theoretically speaking, the state of equilibrium ought only to be established after the lapse of an infinite time, in point of fact the increase of the charge becomes inappreciable after a certain number of hours, which is never very considerable when the dielectric is not very thick; hence it might be proposed to determine experimentally the value of the greatest charge which a given condenser could take when the action of the electrical source is infinitely prolonged. The determination of this limiting charge presents no particular difficulty; it only requires considerable time, seeing that a single experiment may last several days.

I generally worked with small insulating discs with plane surfaces, and with plane metallic armatures applied directly on the solid dielectric.

I first endeavoured to ascertain the relation between the limiting charge and the thickness of the dielectric, and I found that this thickness is quite indifferent. This result appeared to be opposed to that obtained when the air is taken for a dielectric; in this case I have already shown that the charge is almost inversely as the thickness of the layer of air, at least when this thickness is very small in reference to the surface of the armatures. But it is not difficult to explain this divergence, from considerations which I have presented in a previous note. In fact, an air condenser is a simple condenser, while a condenser with a solid dielectric is really a battery by cascade. The imaginary solid dielectric (sulphur, shell-lac, or gutta percha) is a true conductor. The dielectric is really the air which separates the insulating disc from its armatures. Regarding matters in this light, it will be understood that the thickness of the insulating disc between the armatures ought not to influence the limiting charge, but only the time necessary to obtain equilibrium; and in fact, to obtain a limiting charge, the time required is longer as the insulating disc is thicker.

Secondly, I compared fulminating panes of the same dimensions, formed of different dielectrics (sulphur, shell-lac, &c.), and I found that the limiting charge was the same for all bodies; so that it

would be necessary to say that they have all the same inductive capacity—if the use of this phrase (which, as I think, does not express a distinct property) ought not to be completely abandoned.

In all the experiments of which I have hitherto spoken, the armatures of the fulminating panes were directly applied to the insulating disc; in this manner different insulating discs could be compared with each other; but insulating bodies could not be compared with metals. To establish this comparison, I modified the arrangement of the fulminating panes. I separated the metallic armatures from the insulating disc by small layers of air of constant thickness. By working in this manner, and using as intermediate discs metallic plates of different thicknesses, I found that this thickness does not perceptibly influence the charge of the condenser, at least so long as this disc is very thin as compared with the diameter of the discs. I found, secondly (and this point appears to me important), that the limiting charge is not modified by substituting a metallic disc for an insulating one. In the case of an insulating disc, the limiting charge is only obtained after the lapse of a considerable time, varying with the nature and thickness of the insulator, while with the metallic disc the limiting charge is established in an inappreciable time.

When the intermediate disc is an insulator in immediate contact with the armatures, the limiting charge depends, as I have just said, neither on the nature nor thickness of the insulating body; but it may vary considerably with another element, which has not hitherto been taken into account, and which is really an important element: this is the *external resistance* of the insulator. This resistance arises almost entirely, if not quite, from the small layer of air always interposed between the insulating disc and its armatures, even when the surfaces have been planed with the greatest care; and it varies singularly with the hygrometric state of the surrounding air. This circumstance makes the experiments in question somewhat delicate. In fact, from what I have said, when two fulminating panes formed from insulating discs of different nature and thickness are compared, it is indispensable that they be placed in the same hygrometric conditions.

From the whole of my researches, it appeared to follow that insulating solid bodies, submitted to the influence of electricity, behave exactly like metals, and consequently the inductive capacity is not distinct from the conductibility. I am not ignorant that the conditions under which I experimented are extremely different from those indicated by the illustrious philosopher who propounded the notion of inductive capacity. Instead of charging to saturation the Leyden jar with which he worked, Mr. Faraday charged it as rapidly as possible, so as to remove the effects of the conductibility of the dielectric, which he only considers as a disturbing cause. But this mode of view does not seem to me to rest on decisive reasons. I have limited myself, in the research of which I am giving the general results, to considering the *permanent state*; Mr. Faraday, on the contrary, has only occupied himself with the *variable state*; but it is difficult to admit that the distribution of electricity should not be ruled by the same principles in both cases. I propose to revert to the phenomena of the variable state.—*Comptes Rendus*, Sept. 8, 1862.

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AND
JOURNAL OF SCIENCE.

SUPPLEMENT TO VOL. XXV. FOURTH SERIES.

LXVI. *On Asterism and Brewster's Luminous Figures.*
By Prof. VON KOBELL*.

[With Three Plates.]

THE beautiful phenomena of asterism, which were long only known in the case of sapphire and garnet, have been further studied in the investigations of Brewster†, Babinet‡, and Volger§, and have been shown to exist in many minerals. Babinet has described them as phenomena of gratings; and the simpler of them may indeed be easily produced by cutting suitable systems of close parallel lines either in a smooth copper plate, or by ruling them on a glass plate covered with copper or silver. By means of a candle in an otherwise dark room, and with *one* system of such lines, *one* band of light is seen which cuts the lines at right angles; with *two* systems of lines crossing at right angles, a rectangular luminous cross is seen, or, if the bands cut at an acute angle, it is oblique; with *three* systems drawn to the sides of a triangle, a six-rayed luminous star; with radial lines proceeding from a centre, a parhelial circle is seen under certain angles of incidence.

The last phenomenon is often seen through a piece, about an inch long, of an ordinary glass rod (one-third of an inch thick), the ends of which are polished quite smooth. Viewed at a certain distance from a candle-flame, the glass being slightly inclined, a circular luminous ring is visible through these terminal faces, in which the flame always stands in a point of the periphery. In polarized light such glass cylinders show the cross through the faces mentioned. A cylinder of homogeneous glass which does not polarize does not give the phenomenon; but, on the other hand, it is not every polarizing glass which does. In

* Translated from the *Berichte der Königl. Baierischen Acad. der Wissenschaften*, 1862.

† Edinburgh Transactions, vol. xiv. 1837. Phil. Mag. Jan. 1853.

‡ Poggendorff's *Annalen*, vol. xli. 1837.

§ *Sitzungsberichte der Wiener Akademie*, vol. xix. 1856.

crystals and aggregates of crystals a perfectly closed parhelial circle is seldom witnessed. But Professor Plücker has a calcite which even shows through the cleavage-planes two such circles or rings of light, which touch in the image of the flame of the candle, and which, according to the inclination of the crystals, are either seen near one another or in one another. Babinet has ascribed such phenomena to a fibrous structure, and to the corresponding cleavage of the crystal. Volger has noticed that very often the junction-faces of a twin crystal are the cause, and that the asterism of a striated external face often changes when such a face is ground away and then the polished faces are looked through. Neither of them mention the investigations which Brewster made on the subject simultaneously with Babinet, and in which partly he observed corroded faces of natural occurrence, and partly, by superficial corrosion (*Ätzen*), or by rough polishing, made the inner structure active for the light. In this manner Brewster has examined crystals of topaz, garnet, amphibole, axinite, boracite, Liparite, magnetite, amethyst, diamond, and by corrosion crystals of calcite, alum, Liparite, apophyllite, acetate of copper and lime, sulphate of potash, &c.

In the corrosions, in which he used water, hydrochloric acid, nitric acid, and also hydrofluoric acid, he observed that, according to the nature of the corroding agent, the figures are changed, and that by mechanical grinding on a polishing-stone, or with a rasp or file, similar figures are produced, though not pure, and remarkably altered in position as compared with those formed by corrosion. These figures appear in reflected light (of a candle-flame), and also in transmitted light; and if the corroded face is printed in isinglass, may be investigated in transmitted light.

Brewster has investigated more accurately only the crystals of the tesseral, hexagonal, and quadratic systems; he could obtain no definite results for the rhombic, klinorhombic, and klinorhomboidal systems.

The following observations may serve as a contribution to the knowledge of this asterism.

When crystal-faces are to be observed by corrosion, care is to be taken that these faces are smooth and reflecting, and to begin with the feeblest corrosion. For salts very soluble in water, I have used the following process. I moistened a piece of fine cloth with paper and left a part near it dry; I then laid the crystal face upon the dry part, and moved it into the moist part and immediately back again: according to circumstances, this was repeated several times. The cloth is laid on a piece of glass. The observation is made with a candle-flame, and best in a room which is otherwise dark; and the crystal is held between the thumb and fore finger of both hands, close to the candle, in order

that the incidence of the light may be as nearly normal as possible. The crystal is then turned until the image of the reflexion of light is distinctly seen on the surface, and thereby the eye brought as near it as possible. On the table, and at the place above which the crystal is seen, a dead-black paper is laid. If the transparency is enough to allow transmitted light to be seen, the crystal is held with the thumb and forefinger as previously described, so as to cut off the lateral light as much as possible close to the eye, and the flame looked at through it. It is to be observed that the luminous figure is mostly first distinctly seen when standing at a distance of two or three steps from the flame. In judging the luminous image, it is to be considered whether only *one* face, or at the same time its parallel, was corroded, because the latter often gives the picture of the first inverted; thus, for example, in *one* corroded face a three-rayed star is to be seen, but a six-rayed one if the parallel face was also corroded.

The images are very beautiful when the crystalline laminæ are fastened in pieces of cork, and the flame looked at through an opera-glass at a distance of about eight steps, the crystal being held between the eye and the eyepiece.

Such images are easiest produced and observed in alum. When a moist cloth is passed once or twice over a smooth octahedral surface, and then a dry one, a three-rayed star immediately appears in the principal form, like fig. 1, Pl. II.; by frequent moistening, it changes in the centre, and three short rays appear between the first; but the star is immediately changed into the six-rayed one (fig. 2) when the crystal is moistened with dilute nitric or hydrochloric acid in the manner described. I used mostly one volume of concentrated acid, and one or two volumes of water. Further moistening with water (and drying) alters the six-rayed star again into a three-rayed one. Brewster also states that such a corroded surface, upon which triangles are visible, as in fig. 3, reproduce themselves perfectly if the crystal is dipped in a saturated solution of alum, and that the redintegration and filling up of the attacked parts proceeds in this manner with inconceivable rapidity*. I did not quite find it so, yet I obtained normal surfaces when a corroded strip of alum was dipped in a warm and not too concentrated solution of alum and then left to spontaneous evaporation. The faces of the hexahedron and of the rhombic dodecahedron, which often occur in alum in combination with the octahedron, present this phenomenon—that in the

* “The singular fact in this experiment is the inconceivable rapidity with which the particles in the solution fly into their proper places upon the disintegrated surface, and become a permanent portion of the solid crystal.”
—*Loc. cit.* p. 174.

former, by superficial corrosion, a rectangular cross is produced, and in the latter a luminous band lying in the short diagonal of the dodecahedral surface. These images are not changed by hydrochloric acid. The rectangular cross upon the hexahedral surface shows itself parallel to the sides and to the diagonals of the surfaces; the first remains rectangular even with obliquely incident light, the latter becomes acute-angled. Potash-alum, ammonia-alum, and chromé-alum comport themselves quite in the same way. The three-rayed star of the octahedral surfaces are also often seen in natural crystals of Liparite and magnetite.

If an octahedral face of Liparite is rubbed smooth upon a rough broad file, and then the surface purified with water and dried, the three-rayed star is also seen in incident light, the rays directed to the angles of the triangle. On a hemitropic crystal of *nitrate of strontia* an inch long, the star on the octahedron faces was like fig. 4 (corroded with water), and the rays did not proceed at right angles to the junction edge of the octahedral or cubic faces, or to the angles of the octahedral face, but were at an acute angle thereto. By repeated corroding with water the cubic faces showed fig. 5.

In the *quadratic system*, I distinctly observed, upon looking at a candle-flame through the *basal face* of the tabular crystals of the *apophyllite* of Fassa, a luminous cross in the direction of the diagonal; likewise in *ferrocyanide of potassium* when superficially corroded by breathing; in *sulphate of nickel* I saw fig. 6 on the basal surface in reflected light.

On the faces of the quadratic pyramid in *phosphate of ammonia* and *arseniate of potash*, after slight corrosion with water, the reflected image of a three-rayed star is seen; the rays of which, however, do not proceed to the angles of the triangle, as in the case of the octahedron, but towards the sides, and cut under angles of two kinds, as do the normals to these sides.

In the hexagonal system, *calcite*, by corrosion with hydrochloric and nitric acids, presents beautiful phenomena, which Brewster has already partially described. The crystal is dipped in the acid and then in the water, and dried with a soft piece of linen. By immersion in hydrochloric acid (one volume of acid to seven of water) the luminous figure 7 is obtained on the surface of the cleavage-rhombohedron; the short ray *r* proceeding to the corner edge often lengthens like the rest, by being corroded in the manner described, and luminous rays result, becoming broader outwards, which are seen with great beauty in transmitted light. The change which fig. 7 undergoes when the crystal is immersed in nitric acid (diluted with one volume of water) is very surprising; fig. 8 is then seen. By this figure it may easily be recognized whether a crystal has been immersed

in nitric acid or not; and hydrochloric and nitric acid can be distinguished by means of the figures. In such corrosion, microscopic triangles are formed upon the rhombohedral surface, one point of which is directed towards the vertical solid angle, and therefore opposite the ray r . These triangles arise from depressions which correspond to a three-sided pyramid (the vertical top-piece of a rhombohedron).

In transmitted light the phenomena are as follows:—When one face was corroded with hydrochloric acid, a star was seen consisting of three pencils of light becoming broader on the outside; when at the same time the opposite parallel face was corroded, the star appeared six-rayed. If two parallel faces were corroded with nitric acid, on looking through the crystal an acute-angled cross was seen, with spots of light in the obtuse angles. The deportment of the so-called *Streifenspath* is also interesting: it is a hemitrope of a rhombohedron in oscillatorial repetition, where the face of rotation is $-\frac{1}{2}R$. One surface of the cleavage-rhombohedral is striated in the direction of the long diagonal, the rest are smooth. If a candle-flame is looked at from some distance through the latter faces, by holding the principal section (through the vertical solid angle) in a vertical position, rhomboids appear like fig. 9, whose point of intersection show the flame partially with prismatic colours. Through the striated surface this face is only seen distorted. If such a crystal is corroded, the smooth faces also appear corroded like fig. 10, face b and c ; and on looking through against a flame, a band of light appears cutting these lines at right angles, in which the flame appears in spots of light at equal distances.

Volger assumes that all primary forms of calcite are triplets, through a threefold repetition of the above law. The optical deportment does not agree with this; for the crystals in which the hemitropic aggregation mentioned is distinctly visible, exhibit through the basal faces in polarized light entirely peculiar phenomena, which do not occur in the ordinary fundamental forms of calcite. I have described these phenomena in the *Münchner Gel. Anz.* 1855, No. 18.

When the hexagonal prism of calcite is corroded by several immersions in dilute hydrochloric acid, fig. 11, Pl. III. appears on alternate faces, like 1 and 2; the ray going parallel to the axis is turned both above and below towards the vertical edges of the cleavage-rhombohedral. I observed these images in reflected light in two crystals from Andreasberg an inch long. When a face on the cleavage-rhombohedral of calcite is ground smooth by rubbing it with circular motion on a broad file, and then the face cleaned with water and dried, on looking through it at a flame a luminous line is seen in the direction of the short dia-

gonal of the face; on the basal surface a regular three-rayed star is also seen, whose rays are directed to the junction edges with the rhombohedron of cleavage. Sometimes a fourth ray goes through the star, dividing the angle of 120° .

The phenomenon presented by *dolomite* is like that of calcite when a cleavage-piece is corroded with hydrochloric acid, by leaving it for a few days in the acid, or accelerating the action by heating. But the reflected image is different from that of calcite, in that the angle α between the rays is notably obtuser, and that the ray r is very short, and not, as in calcite, turned towards the *lateral edge*, but towards the *vertical edge*. In this corrosion, dull and striated bevelled edges are observed on the vertical faces.

With nitric acid I only obtained distorted pictures; even with rough polishing I could not obtain the band of light as with calcite.

In the *magnesite* of Snarum in Norway, when a cleavage piece is boiled for some time in hydrochloric acid, the image of reflexion is like that of dolomite—though the angle between α is still greater, and the ray r very short but also turned towards the vertical solid angle.

Siderite (from Nassau), when boiled in hydrochloric acid, behaved like dolomite.

In the *rhombic system* I observed in the basal faces of tolerably large crystals of *tartrate of soda and potash*, which had been moistened with water and then rubbed over with a dry cloth, the beautiful reflected figure 12, which by frequent corrosion undergoes many changes, and, on looking through it, appears like fig. 13. When I used water instead of hydrochloric acid, fig. 12 gave place to an indefinite rhombic spot of light, but it immediately appeared again when the surface was rubbed with a moist cloth.

When a prism of *nitroprusside of sodium* (the combination of the rhombic prism of $105^\circ 10'$ with the macro- and brachydiagonal spaces) is very slightly corroded with water, the figures of reflexion are shown as fig. 14 represents them. At a certain angle the rays upon the p faces of the rhombic prism are obtained tolerably large, and only a three-rayed star appears; the crosses belong to the macro- and brachydiagonal faces.

Chloride of potassium and bismuth gives, when corroded by breathing, an oblique cross on the basal face towards the sides of the rhombus of this face, and also a band of light towards the long diagonal; under the same circumstances there is in *chloride of barium* a band of light towards the short diagonal of the ordinary rhombic plates; with further corrosion, spots of light show themselves near it, but no band in the direction of the long diagonal.

In *formate of strontia* there is a cross in the direction of the diagonals of the rectangular tabular crystals. In the tabular crystals of *ferridcyanide of potassium*, when corroded by being breathed upon, there appears a beautiful oblique luminous cross towards the junction edge with the pyramid, and a band at right angles to the axis like fig. 15. By further careful corrosion fig. 16 appears. On the surface small rhombohedra are visible in the position indicated by figs. 15 and 16. The luminous figures are seen of especial beauty in incident light when the crystalline lamina is held in a blackened piece of paste-board.

In the *klinorhombic system*, I could observe beautiful crystals of *sulphate of magnesia and ammonia*. The lateral faces of the prism of $109^\circ, 12^\circ$ show the reflected images Plate IV. fig. 17, and in the following order:—the faces 1 and 2 adjacent to the klinodiagonal principal section on the front side of the hendyœhedron (hence the terminal face inclined towards the observer); the parts of the cross *a* inclined upwards towards the obtuse lateral edge on the terminal face, but *b* inclined downwards; in like manner, but turned in front, these crosses show themselves upon the surfaces 3 and 4 at the back of the hendyœhedron. The isomorphous compounds, *sulphate of nickel and ammonia*, *protosulphate of iron and ammonia*, *sulphate of nickel and potash*, and the corresponding *cobalt-salt*, exhibit a similar deportment.

In a very beautiful crystal of *sulphate of manganese and ammonia*, the right half of the cross-arm *c* in the face 2 was shorter, and limited by an elliptical spot; the left arm of the cross corresponding to face 3 was similar. *Sulphate of oxide of copper and potash* showed this figure of reflexion but indistinctly.

When a plate of *gypsum* is dipped for a few days in water, or for a shorter time in dilute hydrochloric acid, the perfect cleavage plate in reflected and transmitted light shows a beautiful band of light, rectangular or almost rectangular to the cleavage-face, which is characterized by the conchoidal fracture fig. 18.

In the *klinorhomboidal system*, I observed *sulphate of copper*, fig. 19. With a very slight corrosion there was exhibited on the face *p'* a cruciform ray of light, fig. 20; on *p* a band of light (fig. 21) rectangular to the edge of the prism; and on the terminal face *o* the reflected image fig. 22; in a certain direction the trefoil inclined towards the solid angle *ee*. These images were observed in two very beautiful crystals with smooth faces; in general the faces of this crystal are not smooth enough.

I have here only described the faces in which the luminous figures are distinctly shown; in many salts which I further investigated, for instance green vitriol, sulphate of magnesia, white vitriol, chromate of potash, saltpetre, &c., I could not

obtain a definite picture, probably because a less energetic corroding substance than water is necessary.

Leydolt's microscopic observations of corroded surfaces have indeed shown that crystals consist of molecules whose forms belong to the crystalline series of the regularly built aggregate; and the investigations of Volger and Scharff have proved that the structure is manifold and complex: Brewster's luminous figures show this in a still higher degree. How must a position of the molecules and a difference in their particles be constituted, which, as in calcite for example, produces a different result when corroded with nitric acid to what it does when corroded with hydrochloric acid? and if it is not to be doubted that all the lines of these figures indicate striations in directions at right angles to them, what structure can produce the changes which vary with each trace of a further corrosion, and the manifold curves and spots which we perceive on the rhombohedral surfaces of calcite and many other minerals corroded by nitric acid?

Theoretical crystallogeneses stands here, so to speak, before a mirror which shows all the difficulties and puzzles which it must overcome and solve; and at present we cannot expect that it will attain such a solution. Brewster said on this point, "In whatever way crystallographers shall succeed in accounting for the various secondary forms of crystals, they are then only on the threshold of their subject. The real constitution of crystals would be still unknown; and though the examination of these bodies has been pretty diligently pursued, we can at this moment form no adequate idea of the complex and beautiful organization of these apparently simple structures."

LXVII. *Geometrical Proof of the fundamental principle of Laplace's Functions.* By Archdeacon J. H. PRATT*.

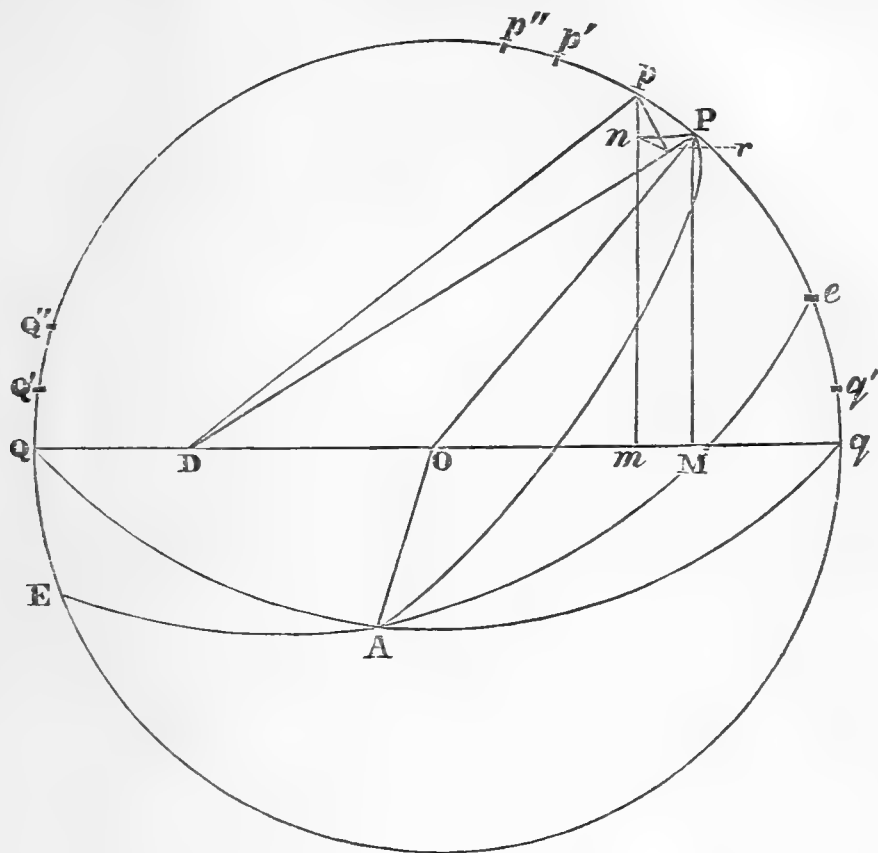
1. **B**Y the fundamental principle of Laplace's Functions, I mean the property expressed by the formula

$$\int_{-1}^1 \int_0^{2\pi} \frac{(1-c^2)F(\mu', \omega') d\mu' d\omega'}{(1+c^2-2cp)^{\frac{3}{2}}} = 4\pi F(\mu, \omega),$$

where $c=1$, and where μ and ω , and μ' and ω' are the polar coordinates (measured as described below) to a fixed point Q and a variable point P, situated on a sphere of radius =1 about the origin of coordinates, and p = the cosine of the angle subtended by Q P at the origin. Let the diagram be so drawn that the plane of the paper passes through the origin of coordinates and the points Q and P. Let A be the pole on the sphere from

* Communicated by the Author.

which the angles $A'OQ = \theta$, $AOP = \theta'$ are measured ($\mu = \cos \theta$, $\mu' = \cos \theta'$), eAE the great circle from which the angles $EAQ = \omega$, $EAP = \omega'$ are measured : $p = \cos QOP$.



Now an element of the surface of the sphere at P

$$= \sin \theta' d\theta' d\omega' = -d\mu' d\omega'.$$

I shall change the origin of the angles from A to Q, and make $QP = \cos^{-1}p$ and $AQP = \psi$ the coordinates to P from Q. Hence, dividing the surface of the sphere into elements about Q as the new origin of angles, the element at P $= -dp d\psi$, and the function to be integrated is

$$\int_{-1}^1 \int_0^{2\pi} \frac{(1-c^2)F(\mu', \omega') dp d\psi}{(1+c^2-2cp)^{\frac{3}{2}}} (c=1);$$

the values of μ' and ω' in terms of p and ψ should be substituted in $F(\mu', \omega')$: this is not done because it will not be wanted.

I shall first take the case in which $F(\mu', \omega')$ is constant and $=1$: then the integral is

$$\int_{-1}^1 \int_0^{2\pi} \frac{(1-c^2) dp d\psi}{(1+c^2-2cp)^{\frac{3}{2}}}.$$

Take the point D in OQ so that OD = c, then DQ = 1 - c, Dq = 1 + c. Join DP and OP, take pQ near to P. Draw PM, pm perpendicular and Pn parallel to Qq, pr perpendicular to DP. Then, because Pnp and Prp are right angles, a circle can be drawn through P, p, n, r. Hence

angle $rnP = \text{angle } rpP$, which ultimately $= \text{angle } OPD$,
also

angle $nPr = \text{angle } PDO$.

Hence the triangles Prn and DOP are similar to each other ;

$$\therefore \frac{DO}{DP} = \frac{Pr}{Pn} = \frac{DP - Dp}{mM} \text{ ultimately,}$$

$$\therefore \frac{DO \cdot mM}{DP^3} = \frac{Dp}{DP} \left(\frac{1}{Dp} - \frac{1}{DP} \right) = \frac{1}{Dp} - \frac{1}{DP} \text{ ultimately.}$$

Now $DO = c$, $mM = d$, $Qm = dp$, $DP^2 = 1 + c^2 - 2cp$; and by supposing the whole semicircle QPq divided into equal divisions (each $= Pp$) at $Q' Q'' \dots p'' p' \dots q'$, and joining $DQ' \dots Dp' \dots$, adding together all the equations like the above one, and taking the limit, in fact integrating, we have

$$\begin{aligned} & \int_{-1}^1 \int_0^{2\pi} \frac{(1-c^2)dp d\psi}{(1+c^2-2cp)^{\frac{3}{2}}} \text{ or } \int_0^{2\pi} \int_{-1}^1 \frac{(1-c^2)d\psi dp}{(1+c^2-2cp)^{\frac{3}{2}}} \\ &= \int_0^{2\pi} d\psi \frac{1-c^2}{c} \left\{ \left(\frac{1}{DQ} - \frac{1}{DQ'} \right) + \left(\frac{1}{DQ'} - \frac{1}{DQ''} \right) + \dots \right. \\ & \quad \left. + \left(\frac{1}{Dp'} - \frac{1}{Dp} \right) + \left(\frac{1}{Dp} - \frac{1}{DP} \right) + \dots + \left(\frac{1}{Dq'} - \frac{1}{Dq} \right) \right\} \\ &= \int_0^{2\pi} d\psi \frac{1-c^2}{c} \left\{ \frac{1}{DQ} - \frac{1}{Dq} \right\} = \int_0^{2\pi} \frac{1-c^2}{c} \left(\frac{1}{1-c} - \frac{1}{1+c} \right) d\psi \\ &= \int_0^{2\pi} 2d\psi = 4\pi. \end{aligned}$$

This simple result (which is remarkable for being independent of c) arises from the second and third, the third and fourth ... terms destroying each other, leaving only the first and last remaining.

2. The more general property is now easily proved. At the point Q , where $\mu' = \mu$ and $\omega' = \omega$, the value of the function $F(\mu', \omega')$ is $F(\mu, \omega)$; call this F ; and suppose $F', F'', F''' \dots F^{(n)}$ to be the values of $F(\mu', \omega')$ at the successive points $Q', Q'', Q''' \dots q$. Hence, when $c = 1$,

$$\begin{aligned} & \int_{-1}^1 \int_0^{2\pi} \frac{(1-c^2)F(\mu', \omega')d\mu' d\omega'}{(1+c^2-2cp)^{\frac{3}{2}}} \text{ or } \int_0^{2\pi} \int_{-1}^1 \frac{(1-c^2)F(\mu', \omega')d\psi dp}{(1+c^2-2cp)^{\frac{3}{2}}} \\ &= \text{limit of } \int_0^{2\pi} d\psi \frac{1-c^2}{c} \left\{ F \left(\frac{1}{DQ} - \frac{1}{DQ'} \right) + F' \left(\frac{1}{DQ'} - \frac{1}{DQ''} \right) \right. \\ & \quad \left. + \dots + F^{(n)} \left(\frac{1}{Dq'} - \frac{1}{Dq} \right) \right\} \text{ when } c = 1, \\ &= \text{limit of } \int_0^{2\pi} d\psi \frac{1-c^2}{c} \left\{ \frac{F}{1-c} + \frac{F' - F}{DQ'} + \frac{F'' - F'}{DQ''} + \dots - \frac{F^{(n)}}{1+c} \right\} \end{aligned}$$

$$= \text{limit of } \int_0^{2\pi} d\psi \left\{ F \frac{1+c}{c} + \frac{1+c}{c} \left(\frac{1-c}{DQ'} (F' - F) + \frac{1-c}{DQ''} (F'' - F') \right. \right. \\ \left. \left. + \dots - \frac{1-c}{1+c} F^{(n)} \right) \right\}, c=1,$$

$$= \int_0^{2\pi} 2d\psi \cdot F(\mu, \omega), \text{ as } \frac{1-c}{DQ'}, \frac{1-c}{DQ''}, \dots \text{ all vanish.}$$

$$= 4\pi F(\mu, \omega), \text{ as } F(\mu, \omega) \text{ is independent of } \psi.$$

This is the celebrated theorem of Laplace, on which his powerful analysis is based.

Calcutta, October 8, 1862.

LXVIII. *Postscript to a Paper "On Tests of the Truth of the Fluid Theory of the Figure of the Earth."* By Archdeacon J. H. PRATT*.

A STILL better hypothetical re-arrangement of the mass of the earth, with a view to test the fluid theory, is the following. All we know *à priori* regarding the mass is this, that the external surface is an oblate spheroid of ellipticity $\frac{1}{294}$, that the mean density is twice that of the surface, and that the materials are arranged in more or less spherical strata about the centre. Suppose, then, that we conceive that the external spheroidal form has not been derived from a former state of fluidity, but from the constant action of the centrifugal force removing the parts discharged by weathering upon the surface towards the equator, and that the interior is not necessarily arranged according to the fluid law. Conceive the mass made up as follows—of a homogeneous spheroid of the same density as the superficial parts of the earth, with the remainder of the mass distributed through it in some unknown arrangement. That arrangement, as one of our data shows, cannot depart much from that of spherical shells around the centre. I will assume, in the first instance, that this is the arrangement, and will find the effect on the pendulum. It is clear that, as the density of the surface is half the mean density of the earth, the mass of the homogeneous spheroid will be half the mass of the earth, and the remainder will be half also. Hence the potential of the earth's mass under this new arrangement will be

$$U = \frac{\frac{1}{2}E}{r} + \frac{\frac{1}{2}Ea^2}{r^3} \left(\epsilon - \frac{m}{2} \right) \left(\frac{1}{3} - \mu^2 \right) + \frac{\frac{1}{2}E}{r} \\ = \frac{E}{r} + \frac{1}{2} \frac{Ea^2}{r^3} \left(\epsilon - \frac{m}{2} \right) \left(\frac{1}{3} - \mu^2 \right).$$

* Communicated by the Author.

Hence the ratio of the increase of gravity between the equator and the poles, under this new arrangement, to gravity itself $= \frac{3}{2}(\epsilon - \frac{1}{2}m)$: and the ratio which this increase bears to the actual increase as determined by pendulum experiments

$$= \frac{3}{2} \left(\frac{1}{294} - \frac{1}{578} \right) \div 0.0051828 = 0.48.$$

The effect, then, would be to reduce everywhere the gain of the seconds' pendulum over the rate at the equator by very nearly one-half the number of beats at present observed.

Experiment shows no sudden changes, nor any marked deviation from a regular increase, varying as the change in the square of the latitude, in the rates of the pendulum in passing from the equator to the poles. Hence the excess of matter above the homogeneous spheroid cannot be distributed irregularly. If we suppose it distributed in exact spherical shells, as above, the effect on the pendulum would be very great, and therefore very perceptible. Any departure in the strata from the spherical form, not towards the oblate spheroids of the fluid theory, but in the opposite direction, would produce a result still more discordant with experiment; whereas every approach in the distribution towards those spheroids will bring the calculation into nearer accordance with fact. No stronger testimony can well be borne to the truth of the fluid arrangement, and therefore also to the fluid theory, as we cannot otherwise conceive what cause can have made the interior strata to bulge out at their equators.

Calcutta, October 27, 1862.

LXIX. *On Aluminium Bronze as a Material for the Construction of Astronomical and other Philosophical Instruments.* By Lieut.-Colonel A. STRANGE, F.R.A.S.*

THE author, after referring to the astronomical and geodesical instruments about to be constructed, under his superintendence, by order of the Government, for the use of the Great Trigonometrical Survey of India, and after noticing that one of the points which has given him most anxiety had been the selection of the proper material or materials of which to construct these instruments, and that his present remarks were confined to the great Theodolite with a horizontal circle 3 feet in diameter, and after detailing the requirements of such an instrument, proceeds as follows:—

“Such then was the problem presented for solution: to construct an instrument with extended powers, and cast as much as

* From the Monthly Notices of the Astronomical Society for Nov. 1862.

possible in masses the transportation of which, over the most difficult ground, should not be beyond the power of human labour.

“When on the point of compromising the difficulty by separating, so as to form distinct packages, parts hitherto regarded as inseparable, my attention was attracted by the various articles made of ‘Aluminium Bronze’ sent to the International Exhibition by Messrs. Bell Brothers, of Newcastle, and M. Morin, of Paris. The inquiries I made in various quarters satisfied me that this metal possessed most valuable qualities, but I failed in my endeavours to obtain reliable numerical data for comparing it with other metals. I therefore instituted experiments on it, the results of which I beg now to communicate to the Society.

“The alloy called aluminium bronze was first, I believe, made by Dr. Percy five or six years ago, and is composed of aluminium and copper in various proportions, 10 per cent. of aluminium, however, giving the best material for mechanical purposes.

“The qualities of most importance in instrument-making are, (1) tensile strength; (2) resistance to compression; (3) malleability; (4) transverse strength or rigidity; (5) expansive ratio; (6) founding-qualities; (7) behaviour under files, cutting-tools, &c.; (8) resistance to atmospheric influences; (9) fitness to receive graduation; (10) elasticity; (11) fitness for being made into tubes; (12) specific gravity.

“Of these, tensile strength, resistance to compression, and malleability were most obligingly tested for me by Mr. Anderson at the Royal Gun Factory, Woolwich; and the other qualities by Messrs. Simms, by whom the great theodolite is now being constructed. I will take the above enumerated properties in their order, premising that to have obtained results of an absolute and final character would have involved more time than I could spare, and that therefore those which follow, though sufficiently reliable for almost any practical purpose, are open to the correction of more extensive experiments.

“(1) *Tensile strength*.—This was tried some years ago by Mr. Anderson with the following results. Breaking-strain,

Aluminium bronze . .	95,747	} lbs. per square inch.
Gun-metal	32,000	

Mr. Anderson was good enough to try it again for me in September last, and states in his report, ‘The average tenacity of this metal proved to be 22 tons 12 cwt. (50,624 lbs.) breaking-weight per square inch in the two specimens tested; elongations did not take place until 4300 lbs. in the one case, and 3600 lbs. in the other, had been applied, when a permanent elongation was noticed of .009 of an inch in the first specimen, and .034 of an

inch in the last.' Mr. Anderson adds that the specimens were not quite sound.

"In the above-cited report Mr. Anderson gives a higher tensile strength to gun-metal than before, namely, 17 tons (38,080 lbs.) for the average of the '*best specimens*' tested at the gun-factories.

"Combining the results, we have the average breaking tensile strength of the two metals as follows :—

$$\left. \begin{array}{l} \text{Aluminium bronze } \frac{95747 + 50624}{2} = 73,185 \\ \text{Gun-metal } \quad \quad \quad \frac{32000 + 38080}{2} = 35,040 \end{array} \right\} \text{lbs. per sq. inch,}$$

the ratio being 1 to 0·48, or rather more than 2 to 1, in favour of aluminium bronze.

"For the purpose of comparing the tenacity of the new alloy with steel, we have data given by Mr. Anderson in a lecture* on Materials for Rifled Cannon, in which he states that cast steel varies in tensile strength from 114,000 to 67,000 lbs. per square inch; but he objects to the higher qualities as liable to brittleness, and prefers, where great strains are in question, an average quality of cast steel breaking at 80,000 lbs.; and he adds that specimens from a gun made of Krupp's famous cast steel, characterized by 'softness' (which Mr. Anderson considers favourable to tenacity) and 'perfect soundness,' gave 72,000 lbs. per square inch, which we see is 1185 lbs. less than the average strength of aluminium bronze above given.

"(2) *Resistance to Compression*.—Mr. Anderson reports on this as follows :—

"The ultimate amount of compression applied was 59 tons 2 cwt. 1 qr. 4 lbs. (132,416 lbs.), under which the specimen † became too much distorted to permit of more weight being applied with any true result. Compression was not perceptible until 9 tons 2 cwt. per square inch (20,384 lbs.) was applied, when the specimen suddenly gave to the extent of ·006 of an inch, and on the weight being removed an elasticity of ·001 was observed, which gives the first permanent compression as ·005 of an inch.'

"The compressive strength of cast iron varies a good deal. That of 'Carron Iron, No. 3,' the highest given in Ure's '*Dictionary of the Arts*,' is 115,542 lbs. per square inch; but it is difficult to compare in this respect two metals whose behaviour

* Published in No. XXIII. Journal, Royal United Service Institution.

† "The specimen subjected to this enormous pressure, distorted though it is, does not exhibit the trace of a fissure. The cohesion of its particles is inviolate."

under compression is so different, cast iron yielding suddenly and almost totally, and the new alloy more gradually and partially. Astronomical instruments, however, are more dependent on the rigidity or resistance to a transverse force than to any other quality.

“(3) *Malleability*.—Mr. Anderson states on this head:—

“ ‘The qualities of this metal for forging-purposes would appear to be excellent; with the exception of the part heated to a red heat in the shade, all show that it is a good workable material under the hammer almost up to melting-point.’

“I may add that there were specimens in the Exhibition, showing that the alloy could be drawn out under the hammer almost to a needle point.

“I come now to the experiments tried by Messrs. Simms.

“(4) *Transverse strength*.—As the absolute determination of the force necessary to break or permanently bend a bar of metal was beyond our appliances, I begged Messrs. Simms to be satisfied with a comparative value of the rigidity of the new alloy; that is, to ascertain the relative resistances of gun-metal, brass, and aluminium bronze, to a force insufficient to produce permanent flexure. This they succeeded in doing, and report as follows:—

“ ‘The same weight applied to three bars altered the index of our instrument as under:

Brass	2·22 divisions.
Gun-metal	0·15 „
Aluminium bronze	0·05 „

“Hence aluminium bronze would appear to be 3 times more rigid than gun-metal, and upwards of 44 times more rigid than brass.

“(5) *Expansive ratio*.—This determination was also a comparative one. Messrs. Simms found that ‘aluminium bronze is less affected by change of temperature than either gun-metal or brass (a little less than gun-metal, and much less than brass).’

“(6) *Founding-qualities*.—Regarding this there is ample experience. The alloy produces admirable castings of any size.

“(7) *Behaviour under files, cutting-tools, &c.*—In this respect, also, it leaves nothing to be desired. It does not clog the file; and in the lathe and planing-machine the tool removes long elastic shavings, leaving a fine bright, smooth surface. Messrs. Simms state,—

“ ‘It can be worked with much less difficulty than steel, and we should think that screws made of it would (notwithstanding the original great cost of the metal) prove in the end less expensive than screws made of steel.’

“(8) *Resistance to atmospheric influences*.—Messrs. Simms state, ‘it does not readily tarnish.’ This, likewise, is entirely a relative question. Absolute inoxidizability, however desirable, is hardly to be expected. Suffice it that the new alloy tarnishes much less readily than any metal usually employed for astronomical instruments, viz., gun-metal, brass, silver, cast iron, or steel.

“(9) *Fitness to receive graduation*.—Messrs. Simms state, ‘Aluminium bronze takes a fine division, and it will not be necessary to inlay another metal, as is usually done, to receive the graduation.’ This opinion is fully justified by the specimen of graduation executed by Messrs. Simms, the lines of which are remarkably pure and equable, characteristics never presented, in the same degree, they inform me, by lines cut on any other *cast* metal. May not this superiority indicate that the alloy in question is peculiarly homogeneous? The lines are very distinct under the microscope, notwithstanding the yellow colour of the metal.

“(10) *Elasticity*.—I possess no direct experiments bearing on this point. But that the alloy has considerable elasticity is unquestionable. I may here state that an eminent Parisian instrument-maker informed me that, of all the wires tried for the suspension of Foucault’s pendulum for illustrating the rotation of the earth, none, not even those of steel, were so durable under that severe ordeal as wires made of aluminium bronze. It would appear, therefore, to be the most proper material for the suspension springs of clock pendulums.

“(11) *Fitness for being made into tubes*.—It admits of every process necessary for this purpose. It can be soldered with either silver or brass solder; it can be rolled into sheet metal; and it can be hammered and drawn. Hitherto telescope tubes, the cones of transit axes, the pillars of altazimuths, &c., have been made almost exclusively of yellow brass, a metal very deficient in rigidity. Gun-metal does not admit of being rolled, and has therefore never been used for the tubular parts of instruments, for which the new alloy seems pre-eminently suitable.

“(12) *Specific gravity*.—The specific gravities of alloys of aluminium and copper, as determined by Messrs. Bell, are

3 per cent. of aluminium	.	.	.	8.691
4 „ „	.	.	.	8.621
5 „ „	.	.	.	8.369
10 „ „	.	.	.	7.689

The last named, which is the best for the purposes now under notice, is very nearly the same as that of wrought iron, and less than that of either brass or gun-metal.

“It appears from these experiments, and from the concurrent testimony of those who have given it a fair trial, that the 10 per cent. aluminium bronze is far superior, not in one or some, but in every respect, to any metal hitherto used for the construction of philosophical apparatus, and that for such purposes it may be employed in the dimensions that would be proper in the case of cast steel. All parts which otherwise would be made of steel may with perfect safety, and even with advantage, be made of the new alloy, particularly such parts as bolts, and fixing, tangent, and micrometer screws. Its hardness and comparative inoxidizability point it out as peculiarly adapted for pivots, axes, and bearings. If employed for receiving the graduation of circles, the necessity for inlaying another metal will be obviated, by which two advantages will be gained: the hammering which forms part of the operation of inlaying, and which, more or less, must cause unequal density and tension in the circle subjected to such treatment, will be dispensed with; and the effect of inequality of expansion in the circle and the inlaid strip will no longer be a cause of apprehension. With respect to the due visibility of divisions cut on this metal, opinions will perhaps differ. I can only say that I should be well content to observe with them.

“The use of this alloy for the construction of the new Great Theodolite is, in the opinion of Messrs. Simms, and in my own, fully justified by what we now know of it; and the effect of using it will be to keep the weight of the instrument within reasonable limits, notwithstanding its possession of means and appliances not hitherto bestowed on such instruments.

“Two points remain—the making of the alloy, and its cost.

“The metal aluminium is at present extracted in England by one firm only, Messrs. Bell Brothers, Newcastle, under a license from M. Deville, the French discoverer of the process. I have met with instances of failure in making the alloy with copper. Two main points only, however, seem to require particular attention. First, extremely pure copper must be used. The best is copper deposited by electricity; but that kind is very expensive; the next best is copper from Lake Superior, which makes an alloy of excellent quality. The ordinary coppers of commerce generally fail, owing, it is said, chiefly to the presence of iron, which appears to be specially prejudicial. The second precaution is to re-melt the alloy two or three times. The first melting of 10 aluminium and 90 copper produces an alloy of excessive brittleness. Each successive melting, up to a certain point determined by the working-, and particularly the forging-properties of the metal, improves its tenacity and strength. It is probable that after several meltings there will remain in combination with

the copper a somewhat smaller proportion of aluminium than 10 per cent.

“The present price of the English-made 10 per cent. aluminium bronze is 6s. 6d. per lb.; but there is reason to believe that, as the process of extracting the aluminium becomes more largely practised, and the demand for the metal increases, the price will fall. The above price is four or five times that of gun-metal. A much smaller quantity, however, of the new alloy than of gun-metal will give the same strength; and when it is considered how small a ratio the cost of the material bears to the cost of workmanship in refined apparatus, it will be found that even at the present price of the new alloy its cost is not prohibitory, whilst the advantages attending its use promise to outweigh the increased expenditure.

“In the foregoing paper, which I am aware is very imperfect and incomplete, I have sought no more than to contribute such an instalment of practical information regarding this remarkable material as may be of service to those who, like myself, contemplate making use of it, of whom I trust there will be many; for by its use we shall, I venture to think, confer on such structures at once greater strength and less weight, and so diminish tension, flexure, and distortion, to an extent calculated sensibly to improve the higher order of instruments of physical research.”

LXX. On the Skew Surface of the Third Order.

By A. CAYLEY, Esq.*

THE skew surface of the third order, or “cubic scrole” (disregarding a certain special form), may be considered as generated by a line which always passes through three directrices; viz. a plane cubic having a node, and two lines, one of them meeting the cubic in the node, the other of them meeting the cubic in an ordinary point. The analytical investigation possesses some interest as an illustration of the analytical theory of skew surfaces in general.

Take for the equation of the cubic

$$(\alpha^3 + \beta^3)xy - (x^3 + y^3)\alpha\beta = 0,$$

which belongs to a cubic having a node at the origin, and passing through the point (α, β) ; and for the equations of the two lines

$$(x - mz = 0, \quad y - nz = 0),$$

$$(x - \alpha = 0, \quad y - \beta = 0).$$

Then, (X, Y, Z) being current coordinates, the equations of the

* Communicated by the Author.

generating line will be

$$\begin{aligned} X &= x + AZ, \\ Y &= y + BZ. \end{aligned}$$

When this meets the line $(X - mZ = 0, Y - nZ = 0)$, we have

$$\begin{aligned} mZ &= x + AZ, \\ nZ &= y + BZ, \end{aligned}$$

and thence

$$x(n - B) - y(m - A) = 0;$$

or, what is the same thing,

$$nx - my - Bx + Ay = 0.$$

And when it meets the line $(X - \alpha = 0, Y - \beta = 0)$, we have

$$\begin{aligned} \alpha &= x + AZ, \\ \beta &= y + BZ; \end{aligned}$$

and thence

$$B(x - \alpha) - A(y - \beta) = 0.$$

We have thus the system of equations

$$\begin{aligned} (\alpha^3 + \beta^3)xy - (x^3 + y^3)\alpha\beta &= 0, \\ X &= x + AZ, \\ Y &= y + BZ, \\ nx - my - Bx + Ay &= 0, \\ B(x - \alpha) - A(y - \beta) &= 0; \end{aligned}$$

from which, eliminating (A, B, x, y) , we obtain the equation of the surface.

Writing in the last equation

$$B = s(x - \alpha), \quad A = s(y - \beta)$$

(values which give $Bx - Ay = -s(\beta x - \alpha y)$), we find

$$\begin{aligned} X + \alpha sZ &= (1 + sZ)x, \\ Y + \beta sZ &= (1 + sZ)y, \\ (n + \beta s)x - (m + \alpha s)y &= 0; \end{aligned}$$

whence also

$$(n + \beta s)(X + \alpha sZ) - (m + \alpha s)(Y + \beta sZ) = 0,$$

that is,

$$nX - mY + (n\alpha - m\beta)sZ + s(\beta X - \alpha Y) = 0;$$

or eliminating s from this equation and the two equations

$$\begin{aligned} x - X + Z(x - \alpha)s &= 0, \\ y - Y + Z(y - \beta)s &= 0, \end{aligned}$$

we have

$$\{(n\alpha - m\beta)Z + \beta X - \alpha Y\} (x - X) - Z(x - \alpha)(nX - mY) = 0,$$

$$\{(n\alpha - m\beta)Z + \beta X - \alpha Y\} (y - Y) - Z(y - \beta)(nX - mY) = 0,$$

which give

$$\begin{aligned}\Omega x &= X \{(n\alpha - m\beta)Z + \beta X - \alpha Y\} - \alpha Z(nX - mY) \\ &= -mZ\beta X + X(\beta X - \alpha Y) + mZ\alpha Y \\ &= (X - mZ)(\beta X - \alpha Y),\end{aligned}$$

and

$$\begin{aligned}\Omega y &= Y \{(n\alpha - m\beta)Z + \beta X - \alpha Y\} - \beta Z(nX - mY) \\ &= nZ\alpha Y + Y(\beta X - \alpha Y) - nZ\beta X \\ &= (Y - nZ)(\beta X - \alpha Y),\end{aligned}$$

where

$$\begin{aligned}\Omega &= (n\alpha - m\beta)Z + (\beta X - \alpha Y) - Z(nX - mY) \\ &= \beta(X - mZ) - \alpha(Y - nZ) - Z\{n(X - mZ) - m(Y - nZ)\} \\ &= (\beta - nZ)(X - mZ) - (\alpha - mZ)(Y - nZ).\end{aligned}$$

So that

$$\begin{aligned}x &= \frac{(X - mZ)(\beta X - \alpha Y)}{(\beta - nZ)(X - mZ) - (\alpha - mZ)(Y - nZ)}, \\ y &= \frac{(Y - nZ)(\beta X - \alpha Y)}{(\beta - nZ)(X - mZ) - (\alpha - mZ)(Y - nZ)};\end{aligned}$$

which equations give the coordinates (x, y) of the point in which the generating line through the point (X, Y, Z) of the surface meets the cubic

$$(\alpha^3 + \beta^3)xy - (x^3 + y^3)\alpha\beta = 0.$$

Substituting these values of (x, y) in the equation of the cubic, we obtain the equation

$$\begin{aligned}(\alpha^3 + \beta^3)(X - mZ)(Y - nZ) \{(\beta - nZ)(x - mZ) - (\alpha - mZ)(Y - nZ)\} \\ - \alpha\beta(\beta X - \alpha Y) \{(X - mZ)^3 + (Y - nZ)^3\} = 0;\end{aligned}$$

or, as it may be written,

$$\begin{aligned}(\alpha^3 + \beta^3)(X - mZ)(Y - nZ) \{\beta(X - mZ) - \alpha(Y - nZ)\} \\ + (\alpha^3 + \beta^3)(X - mZ)(Y - nZ)Z(mY - nZ) \\ - \alpha\beta(\beta X - \alpha Y) \{(X - mZ)^3 + (Y - nZ)^3\} = 0.\end{aligned}$$

This equation contains, however, the extraneous factor

$$\beta(X - mZ) - \alpha(Y - nZ).$$

which, equated to zero, gives the equation of the plane through

the node and the line ($x-mz=0, y-nz=0$). In fact, assuming

$$\begin{aligned} & (\alpha^3 + \beta^3)(X-mZ)(Y-nZ)Z(mY-nZ) \\ & - \alpha\beta(\beta X - \alpha Y) \{ (X-mZ)^3 + (Y-nZ)^3 \} \\ & = \{ \beta(X-mZ) - \alpha(Y-nZ) \} \Phi(X, Y, Z), \end{aligned}$$

it will presently be shown that Φ is an integral function. Hence, omitting the factor in question, we have

$$(\alpha^3 + \beta^3)(X-mZ)(Y-nZ) + \Phi(X, Y, Z) = 0,$$

which is the equation of the surface. It only remains to find Φ : writing for this purpose $X+mZ, Y+nZ$ in the place of X, Y , respectively, and putting for a moment

$$\Phi(X+mZ, Y+nZ, Z) = \Phi',$$

we have

$$\begin{aligned} & (\alpha^3 + \beta^3)XYZ(mY-nZ) - \alpha\beta \{ \beta(X+mZ) - \alpha(Y+nZ) \} (X^3 + Y^3) \\ & = (\beta X - \alpha Y) \Phi'; \end{aligned}$$

that is,

$$\begin{aligned} & (\beta X - \alpha Y) \Phi' = Z \{ (\alpha^3 + \beta^3)XY(mY-nZ) - (X^3 + Y^3)\alpha\beta(m\beta - n\alpha) \} \\ & - (\beta X - \alpha Y)\alpha\beta(X^3 + Y^3); \end{aligned}$$

or, effecting the division

$$\Phi' = Z \{ (X^2\alpha - Y^2\beta)(\alpha n - \beta m) - XY(\alpha^2 m + \beta^2 n) \} - \alpha\beta(X^3 + Y^3),$$

and then writing $X-mZ, Y-nZ$ in the place of X, Y respectively, we have

$$\begin{aligned} \Phi(X, Y, Z) = & Z \{ ((X-mZ)^2\alpha - (Y-nZ)^2\beta)(\alpha n - \beta m) \\ & - (X-mZ)(Y-nZ)(\alpha^2 m + \beta^2 n) \} - \alpha\beta \{ (X-mZ)^3 + (Y-nZ)^3 \}. \end{aligned}$$

And, finally, the equation of the surface is

$$\begin{aligned} & (\alpha^3 + \beta^3)(X-mZ)(Y-nZ) - \alpha\beta \{ (X-mZ)^3 + (Y-nZ)^3 \} \\ & + Z \{ ((X-mZ)^2\alpha - (Y-nZ)^2\beta)(\alpha n - \beta m) \\ & - (X-mZ)(Y-nZ)(\alpha^2 m + \beta^2 n) \} = 0, \end{aligned}$$

which is, as it should be, of the third order.

Arranging in powers of Z and reducing, the equation is found to be

$$\begin{aligned} & (\alpha^3 + \beta^3)XY - \alpha\beta(X^3 + Y^3) \\ & + Z \{ -(\alpha^3 + \beta^3)(mY + nX) + (X^2\alpha + Y^2\beta)(m\beta + n\alpha) \\ & + \alpha\beta(mX^2 + nY^2) - (\alpha^2 m + \beta^2 n)XY \} \\ & + Z^2 \{ mn(\alpha^3 + \beta^3 - \alpha^2 X - \beta^2 Y) + (\beta n^2 - \alpha m^2)(\beta X - \alpha Y) \} = 0. \end{aligned}$$

The first form puts in evidence the nodal line

$$(X-mZ=0, \quad Y-nZ=0),$$

and the second form puts in evidence the simple line

$$(X - \alpha = 0, \quad Y - \beta = 0).$$

But to obtain a more convenient form, write for a moment $X - mZ = P$, $Y - nZ = Q$. The equation is

$$(\alpha^3 + \beta^3)PQ - \alpha\beta(P^3 + Q^3) + Z\{(P^2\alpha - Q^2\beta)(n\alpha - m\beta) - PQ(m\alpha^2 + n\beta^2)\} = 0,$$

or, as this may be written,

$$= (\alpha^3 + \beta^3)PQ + (\alpha^2P - \beta^2Q)Z(Pn - Qm) + \alpha\beta\{-P^3 - Q^3 - Z(mP^2 + nQ^2)\} = 0;$$

or, observing that $X = P + mZ$, $Y = Q + nZ$, and thence

$$PY - QX = Z(Pn - Qm),$$

$$XP^2 + QY^2 = P^3 + Q^3 + Z(mP^2 + nQ^2),$$

the equation becomes

$$(\alpha^3 + \beta^3)PQ + (\alpha^2P - \beta^2Q)(PY - QX) - \alpha\beta(P^2X + Q^2Y) = 0,$$

or, what is the same thing,

$$(\alpha P^2 - \beta Q^2)(\alpha Y - \beta X) + PQ(\alpha^3 + \beta^3 - \alpha^2X - \beta^2Y) = 0;$$

whence, making a slight change in the form, and restoring for P , Q their values, the equation is

$$\{\alpha(X - mZ)^2 - \beta(Y - nZ)^2\} \{\alpha(Y - \beta) - \beta(X - \alpha)\} - (X - mZ)(Y - nZ) \{\alpha^2(X - \alpha) + \beta^2(Y - \beta)\} = 0,$$

a form which puts in evidence as well the simple line

$$(X - \alpha = 0, \quad Y - \beta = 0)$$

as the nodal line ($X - mZ = 0$, $Y - nZ = 0$). If $Z = 0$, we have

$$(\alpha X^2 - \beta Y^2)(\alpha Y - \beta X) - XY \{\alpha^2(X - \alpha) + \beta^2(Y - \beta)\} = 0,$$

which is in fact the cubic curve

$$(\alpha^3 + \beta^3)XY - \alpha\beta(X^3 + Y^3) = 0.$$

Reverting to a former system of equations

$$nx - my - Bx + Ay = 0,$$

$$B(x - \alpha) - A(y - \beta) = 0,$$

or, as these may be written,

$$Bx - Ay = nx - my,$$

$$B\alpha - A\beta = nx - my,$$

we find

$$B(\beta x - \alpha y) = (\beta - y)(nx - my),$$

$$A(\beta x - \alpha y) = (\alpha - x)(nx - my);$$

so that we have

$$X = x + \frac{(\alpha - x)(nx - my)}{\beta x - \alpha y} Z,$$

$$Y = y + \frac{(\beta - y)(nx - my)}{\beta x - \alpha y} Z$$

as the equations of the generating line which passes through the point (x, y) of the cubic curve.

2 Stone Buildings, W.C.,
October 28, 1862.

LXXI. *Chemical Notices from Foreign Journals.*

By E. ATKINSON, *Ph.D., F.C.S.*

[Continued from p. 390.]

MITSCHERLICH has contributed* an article on spectrum analysis, in which he describes a method for obtaining a constant and intense coloured flame of any particular substance.

A tube closed at the top, but drawn out and somewhat bent upwards at the bottom, contains a solution of the substance to be investigated. The shape of the tube is like that of a Cooper's mercurial receiver. In the drawn-out part a bundle of very fine platinum wires is placed, projecting somewhat out of the tube. This platinum wire is placed in the flame of a Bunsen's burner, and the bundle of platinum wires, in virtue of capillary attraction, remains filled with the liquid, which replaces that which has been evaporated. When the tubes have been in use some time, the bundle of wires becomes choked up with substance, which thereby prevents the absorption of any more. To retain the capillarity, acetate of ammonia is added to the solutions to be examined. This, on burning, disperses the substance under investigation, thereby producing an intense and constant flame. A mixture of 20 parts of a solution of 15 per cent. of acetate of ammonia, and one part of the strongest solution of the salt, is found to be the best. Attention must be paid to the position of the platinum wires, in order that neither too much nor too little liquid reaches the flame.

A series of such tubes containing different substances may be arranged on a stand like those on which pipettes are supported, and brought at pleasure into the flame. The apparatus when not in use may be placed under a glass jar, so as to protect the solutions in the wires from evaporation. The size of the vessels used by Mitscherlich gave a flame which was perfectly bright and lasted for two hours.

* Poggendorff's *Annalen*, July 1862.

To investigate two spectra which are not very complicated, two tubes containing the respective solutions are brought near one another into the flame of a Bunsen's burner. But if the spectra are very complicated, or if other circumstances prevent their being simultaneously introduced into the flame, the following arrangement is adopted:—Two Bunsen's burners are placed in front of one another before the slit. Over that burner nearest the slit a piece of iron plate is fixed somewhat larger than the flame, and about level with the centre of the flame. In this way the upper half of the second flame sends its light over the other; and by placing the platinum bundles, of glasses containing two different solutions, one in the lower half of the flame nearest to the slit, and the other in the upper half of the other, two different spectra may be simultaneously produced.

In a mixture of alkaline earths, where the lime greatly predominates, the presence of baryta and strontia in the spectroscopie is masked, and cannot be discriminated with certainty when their quantity is less than $\frac{2}{1000}$ of the lime. According to Engelhardt*, they may readily be detected by concentrating the baryta and strontia in a quantity of lime. When either carbonate of baryta or strontia is mixed with a large quantity of carbonate of lime, it passes into the caustic state much more easily than if it be pure. When such a product of ignition is boiled out with water, all the baryta and strontia are dissolved, and but little of the lime. A solution of 1000 parts of chloride of calcium and 1 part of chloride of barium, precipitated by carbonate of ammonia, the precipitate ignited and treated with water, gave a solution in which all the baryta lines, even the two feeble bluish ones, could be seen. And a similar mixture containing strontia gave the same reactions, only more intense.

The simplest mode of treatment consists in placing the carbonate to be tested in a platinum crucible, and heating it for a few minutes in a Bunsen's blast-lamp. The mass is then treated with boiling water, filtered, and evaporated, hydrochloric acid having been added, and tested in the spectroscopie in the usual manner.

Schönbein has made a contribution† on the formation of nitrite of ammonia from atmospheric air and moisture under the influence of heat. On a previous occasion he had shown that when phosphorus oxidized in the air, nitrite of ammonia was formed, and had thence concluded that under these circumstances the salt must have been formed from water and atmospheric nitrogen.

* Liebig's *Annalen*, October 1862.

† Ibid.

He adduces the following experiments in support of his view. An open platinum crucible is heated so strongly that a drop of water let fall upon it evaporates at once without showing Leidenfrost's phenomenon. This process is repeated with the purest water, waiting each time till the last drop is evaporated. If now a cold flask is held over the vapour thus formed until a few grammes of water are collected, it will be found that this liquid has the power, when acidulated, of rendering iodides and starch blue. Schönbein states that this experiment is not always successful, and ascribes this to the difficulty of exactly hitting that temperature at which the substance is formed and not destroyed.

To prepare large quantities of such nitrite solution, a copper retort is fitted, such as is used for the distillation of water. The retort is first heated, so that water projected upon it immediately evaporates; this process is repeated as often as the previous quantity is evaporated, and, the retort being connected by its head with the condenser, a quantity of water is collected which has all the properties of a solution of nitrite of ammonia.

Schönbein holds that nitrite of ammonia is always formed, provided that all the conditions for its formation are fulfilled, which are the presence of heat, water, and atmospheric air. Saussure found that ammonia as well as nitrous acid were formed when hydrogen burnt in air, and Schönbein had shown (in 1845) that in the combustion of hydrocarbons and fats an oxidizing substance was produced which at that time he was not able to identify with nitrous acid.

He has found that nitrite is produced whenever charcoal is burnt. On the top of a small charcoal furnace a tightly fitting lid was placed; in the side of the furnace, about 2 inches below the lid, an iron tube an inch in diameter and about 4 inches long was placed, through which the heated air made its exit and passed into a receiver containing about 4 ounces of water. Not more than a pound of charcoal was burnt at a time, and only a sufficient quantity added to keep up the combustion, which must not be too violent, as otherwise a quantity of the product formed is again decomposed.

In the combustion of coal, nitrite is produced; but as the coal always contains sulphur, it produces sulphurous acid, which decomposes the nitrous acid with the formation of sulphuric acid, so that very little nitrite is perceptible. Schönbein suspended a wet sponge in the chimney of a coal-fire for half a day, and found that the water squeezed from it contained distinct quantities of sulphate of ammonia, but also some nitrite.

It is not difficult to understand that in the combustion of certain substances no nitrite is perceived, though it may have

been produced. This is the case when the combustible produces a strong acid, phosphorus for example, in which case the acid formed expels the NO^3 and combines with the ammonia. Schönbein shows that, when small pieces of phosphorus are burnt in a bell-jar over a plate containing water, the water always contains traces of ammonia. He has also found that the combustion of arsenic, which takes place at 200°C. , is attended with the formation of ammonia, and that the same is the case with the combustion of sulphur.

Schönbein thinks that in all these cases the combustion of a body produces nitrite of ammonia only in so far that it is a source of heat, and that the process of oxidation in and for itself has nothing to do with the production of the nitrite. He holds in general that nitrite of ammonia is always produced whenever a space containing aqueous vapour and atmospheric air is in any way suitably heated.

The Messrs. Rogers showed, in 1850, that carbon, even in the difficultly combustible substance graphite, could be oxidized completely by means of a mixture of chromate of potash and sulphuric acid; and Brunner subsequently improved the method. Ullgren has made a communication* on the subject. The method has the inconvenience that it is difficult to tell when the whole of the carbon is oxidized; for when strong sulphuric acid and chromate of potash are used, as is necessary for graphite, an almost insoluble anhydrous chrome-alum is deposited, which partly prevents the oxidation, and partly prevents any unoxidized particles from being seen. This evil is remedied by using *chromic acid* instead of chromate of potash. During the operation, too, a large quantity of oxygen gas is disengaged along with the carbonic acid, and hence it is necessary to pay special attention to the complete absorption of the latter. This is effected in the following manner. About one part of caustic potash is dissolved in four parts of water kept heated at 100° , and pumice in small pieces is added and stirred with it until the whole mass is nearly dry, when it is transferred to stoppered bottles. With this material the absorption-tubes are filled. It is excellently adapted for the purpose, as it absorbs carbonic acid even when disengaged with extreme rapidity.

Ullgren gives a description of the apparatus which he uses for effecting the oxidation, for which the original paper must be referred to, where a figure of it is given. It consists essentially of a small flask in which the oxidation is effected, of a cylinder containing pumice saturated with sulphuric acid, and also a chloride-of-calcium tube to retain the moisture, and a U-tube con-

* Liebig's *Annalen*, October 1862.

taining potash prepared as described above. The difference between the weight of this after an experiment and before, gives the weight of carbonic acid produced by the oxidation of the carbon.

With some kinds of carbon perfect oxidation is very slowly effected; in such cases it is better first to separate all carbon with neutral sulphate of copper, and then to oxidize the residue, which consists of carbon and copper with chromic and sulphuric acids. The author has found that the carbon which is contained in chemical union with iron, as in white cast iron, is very difficult to oxidize. To this question, as well as to the examination of the carbon in steel and in wrought iron, the author intends to return. With respect to the quantities used, there must in general be sixteen parts by weight of chromic acid for each part of the carbonaceous substance to be oxidized, and for each gramme of chromic acid 1.5 cubic centim. of strong sulphuric acid should be used. Water should in most cases be added, in the proportion of 2 cubic centims. to 3 or 4 cubic centims. of sulphuric acid.

From recent experiments which have established the presence of nitrogen in cast iron, steel, and wrought iron, it appears that it is present in two conditions: in one case it is liberated as ammonia when iron is dissolved in hydrochloric acid, inasmuch as the nitrogen unites with hydrogen in *statu nascenti*; and in the other it is left in the carbonaceous mass which remains when iron is dissolved in hydrochloric acid, or when it is heated with sulphide of mercury. It is very probable that the presence of nitrogen greatly affects different qualities of iron, and an easy and certain means of determining its presence is a desideratum. To supply this, Ullgren has undertaken a research the results of which he has published*.

When iron is dissolved in hydrochloric acid, a certain quantity of the ammonia formed escapes along with the vapours. The quantity which thus escapes depends on the quantity and concentration of the acid; in all cases where the nitrogen is to be determined by dissolving iron in HCl, the ammonia which escapes must be separately collected and taken into account. The solution in hydrochloric acid may, however, be avoided, and the ammonia determined in one solution by dissolving the iron in solution of neutral chloride of copper. In one case two grammes of cast iron were treated with a solution of ten grammes sulphate of copper and five of chloride of sodium; when the solution was effected, the liquid was distilled with caustic lime and the am-

* Liebig's *Annalen*, October 1862.

monia collected and determined in the usual way. The iron was thus found to contain 0.103 per cent. of nitrogen,—a result which agreed very closely with that obtained by determining the ammonia which escaped when the same quantity of the same iron was dissolved in HCl, and by determining that remaining in the liquid.

To determine the nitrogen which remains in the residual iron graphite when iron has been dissolved, Boussingault has proposed its conversion into ammonia by treatment with soda-lime. But this method, as Ullgren found, is not accurate, inasmuch as the temperature at which iron graphite is attacked is above that at which ammonia begins to decompose. Nor were experiments more successful which he made to convert the nitrogen into cyanogen by heating the iron graphite with pure carbonate of baryta to a high temperature. He thinks that the nitrogen can only be determined as gas; and the best method he has found is by means of *sulphate of mercury*. This is well adapted for those carbonaceous substances which do not volatilize or decompose at an incipient red heat, and it sustains the combustion both with the oxygen of the oxide of mercury and of that of the sulphuric acid, which is thereby reduced to sulphurous acid.

The execution of the method is briefly as follows:—A combustion-tube closed at one end is provided at the other with a delivery-tube which dips under a graduated glass tube receiver. In the posterior end of the tube, either some magnesite or some bicarbonate of soda, which readily part with their carbonic acid, is placed; after this an asbestos plug, and then the substance mixed with sulphate of mercury and introduced with the usual precautions; then comes another asbestos plug; while the rest of the tube is filled with pumice which has been boiled with concentrated solution of bichromate of potash, allowed to cool, drain, and introduced while still moist. In this condition it completely absorbs sulphurous acid. The other gases are absorbed by means of caustic potash and turmeric. For this purpose the graduated absorption-tube is filled with mercury, inverted over the mercurial trough, and some of the solution introduced by a pipette.

The combustion-tube being placed in a combustion furnace, the posterior end of the tube is heated so as to expel some carbonic acid; when the tube is quite full of it, which is determined by obvious methods, the delivery-tube is placed under the absorption-tube, and the part of the combustion-tube containing the substance then rapidly raised to redness and maintained in that state until the disengagement of gas ceases. The rest of the carbonate is then heated until the combustion- and delivery-tube are filled with carbonic acid. The gas is then measured with the usual precautions.

In this way a specimen of the graphite from the iron used in a previous experiment was found to contain a quantity of nitrogen equal to 0.605 per cent. of the iron, which, with the quantity of nitrogen found as ammonia, 0.103 per cent., makes 0.708 per cent.

The method of determining carbon in iron by dissolving it in chloride of copper always gives too high results, because the carbon, thus separated, contains nitrogen. If the quantity of nitrogen in the graphite be added to the quantity of carbon found by the chromic acid method, that is, $4.332 + 0.605$, the total, 4.937, is very near the quantity of carbon, namely 4.86, found by means of chloride of copper.

To determine the quantity of carbon in cast iron, Wich* adopts the following method. He makes the iron the positive electrode of a Bunsen's battery which is placed in hydrochloric acid, the other electrode being platinum. When the current is too strong, carburetted hydrogen is disengaged, and in that case the current must be moderated by removing the platinum electrode to a greater distance. In a few hours five to six grammes of cast iron are dissolved, and the carbon remains and has the original form of the metal.

M. Kuhlmann, jun., has described† several compounds of thallium with organic acids. The starting-point for their preparation was carbonate of thallium, which was prepared by adding baryta water to a solution of sulphate of thallium until there was no more precipitate. The liquid was then saturated with carbonic acid, the excess of carbonic acid driven off, on which the carbonate of thallium was obtained by slow evaporation in fine elongated flat plates.

The organic salts of thallium resemble potash or soda-salts, excepting the ferro-, the ferricyanide, and the picrate. They are mostly soluble in water, and crystallize easily. They are generally anhydrous, and but little soluble in ether or in alcohol. In analysing these salts various methods were adopted. The cyanides were analysed by determining the nitrogen and hydrogen, and, in the sulphocyanide, the sulphur.

The very soluble salts were changed into the chloride of thallium by passing hydrochloric acid through the solutions. Another method consisted in making use of the insolubility of platino-chloride of thallium, $\text{Pt Cl}^2\text{Tl Cl}$, which is more insoluble in water than the potassium-salt.

The author prepared the oxalate, $\text{C}^4\text{Tl}^2\text{O}^8$, and the acid oxalate,

* *Répertoire de Chimie*, February 1862.

† *Comptes Rendus*, October 13, 1862.

$C^4 H Tl O^8$; the acid tartrate, $C^8 H^5 Tl O^{12}$; the paratartrate, the malate, the citrate, the formate, the acetate, valerianate, benzoate, urate, picrate, the cyanide, and the sulphocyanide, $Cy Tl S^2$, and the cyanate, $C^2 N Tl O^2$.

M. de la Provostaye has given a crystallographical description of these salts.

To prepare perfectly pure oxygenated water, Duprey recommends* that a very rapid current of carbonic acid be passed through distilled water, and from time to time small quantities of finely-powdered peroxide of barium projected into the liquid; when the quantity of carbonate of baryta formed is so great as to hinder the passage of the gas, the clear liquid is decanted and the operation repeated. In this way a solution is obtained which is strongly charged with pure neutral binoxide of hydrogen, which may be concentrated under the air-pump receiver. It is necessary that the current of gas be very rapid, so that it be in excess as regards the binoxide.

Chevreul† has made some experiments on the effects of this solution on vegetable colours, and finds that it acts upon them like chlorine, only more slowly.

At a very high temperature, Caron's alloy of zinc and calcium‡ can, according to Wöhler§, be made to form with charcoal a carbide of calcium, of which a fuller description will be given. This compound has the remarkable property that, when it is treated with water, it decomposes into hydrate of lime and acetylene gas. The gas was not analysed, but there could be no doubt of its identity; for it burns with a luminous fuliginous flame, is decomposed by chlorine even in diffused light with separation of carbon, and precipitates an explosive compound from an ammoniacal solution of oxide of silver.

M. Bechamp|| has published an investigation on xyloidine, the body obtained by the action of nitric acid on starch, and on some nitric derivatives of starch. He has shown that xyloidine is a mononitric derivative of starch. It is best obtained by mixing one part of dry starch in a mortar with 5 to 8 parts of fuming nitric acid, until the whole is reduced to a perfectly transparent semifluid mass, to which 20 to 30 parts of cold distilled water are then added. A granular insoluble powder is thereby separated, which is washed, dried, dissolved in a mixture of equal parts of crystallizable acetic acid, and of terhydrated acetic acid, and the

* *Comptes Rendus*, November 10, 1862.

† Ibid.

‡ *Phil. Mag.* S. 4. vol. xx. p. 376.

§ *Liebig's Annalen*, November 1862.

|| *Annales de Chimie et de Physique*, March 1862.

mixture filtered. To the filtrate a small quantity of water is added, which separates a viscous mass; this, treated with a large quantity of water, is resolved into a white powder, which when dried is quite homogeneous.

The mass thus obtained is insoluble in alcohol or ether, or in a mixture of the two.

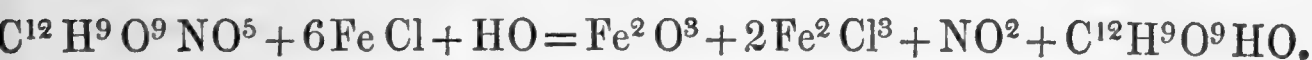
If in the above process a larger quantity of nitric acid be used, and the temperature be not allowed to sink too rapidly, a perfectly transparent solution is obtained, which thrown into water gives a less caseous and more pulverulent precipitate. This, washed and dried, constitutes a new compound; for, unlike the other, it is soluble in a mixture of alcohol and ether.

Both these substances have the composition $C^{12} H^9 NO^{14}$, or $C^{12} H^9 O^9 NO^5$: Bechamp names the former *mononitric* starch, and the latter *isomononitric* starch.

A dinitric compound is obtained by dissolving starch in 12 parts of nitric acid, and then pouring into it 8 parts of concentrated sulphuric acid. The whole mass is then poured into a large quantity of water, by which a pulverulent product is separated, which is thoroughly washed and dried. This body consists of dinitric starch in two molecular modifications: one of these is insoluble in strong alcohol, but soluble in a mixture of alcohol and ether; the other, *isodinitric starch*, is soluble in alcohol. The composition of both is expressed by the formula $C^{12} H^8 O^8 2NO^5$.

These substances when heated give off red vapours and de-flagrate; the mononitric compounds are more stable than the dinitric; and the insoluble modification more so than the soluble.

When one of these compounds is heated with protochloride of iron the starch is regenerated, bin oxide of nitrogen is liberated, and peroxide of iron deposited. The reaction in the case of the mononitric compound is this:



The body produced has all the properties of ordinary soluble starch.

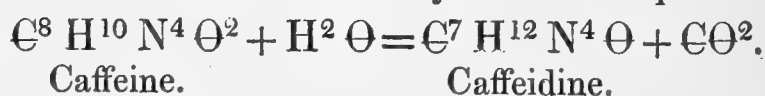
Soluble starch is strongly dextrogyrate; and the nitric derivatives are also dextrogyrate, but with an intensity decreasing with the quantity of nitric acid in the compound.

M. Bechamp considers these compounds as nitrates analogous to nitrate of potash or nitric ether.

The formula of quinine, $C^{20} H^{24} N^2 O^2$, only differs from that of cinchonine, $C^{20} H^{24} N^2 O$, in containing one atom more oxygen. The same relations obtain as between glycolic acid, $C^2 H^4 O^3$, and

acetic acid, $\text{C}^2 \text{H}^4 \text{O}^2$. Kekulé has shown* that the latter acid may be transformed into the former by a method which has since then received extensive applications to other acids. It consists in first replacing an atom of hydrogen by chlorine, iodine, or bromine, and then treating this body by potash, or oxide of silver and water, by which the atom of hydrogen is replaced by HO . H. Strecker† has applied this reaction to cinchonine in the expectation of changing it into quinine. Bibrominated cinchonine, $\text{C}^{20} \text{H}^{22} \text{Br}^2 \text{O}$, was prepared by the action of bromine on hydrochlorate of cinchonine. This was treated with alcoholic potash until all bromine was removed, the excess of potash was saturated with carbonic acid, the solution evaporated to dryness, and the salts soluble in water extracted by treatment with water. On treating the residue with boiling alcohol a substance was dissolved out, which crystallized, on cooling, in colourless laminæ. This substance is a base; its salts crystallize in general with difficulty; its formula is that of quinine, but it is only isomeric with that substance; its solutions do not become green when treated with hydrochloric acid and ammonia, nor do they fluoresce like quinine solutions. From analogy with similar substances, this new base may be called *oxycinchonine*.

Wurtz found that when caffeine was treated with potash, methylamine was given off; but he did not investigate the other products of the reaction. Strecker‡ has examined the decomposition of caffeine by baryta. When heated in a retort connected with a condenser, methylamine and ammonia are given off, and some carbonate of baryta formed. Besides methylamine and ammonia no volatile bodies are formed. The mass in the retort was filtered, supersaturated with sulphuric acid, filtered from the sulphate of baryta, and the filtrate evaporated in the water-bath. On cooling, crystals are formed, the quantity of which increased on the addition of alcohol. These crystals are the sulphate of a new base, which Strecker calls *caffeidine*. From the analysis of its sulphate, the formula of the base was found to be $\text{C}^7 \text{H}^{12} \text{N}^4 \text{O}^2$, and its formation from caffeine may be thus expressed:



The occurrence of methylamine and ammonia depends doubtless upon a further decomposition of the caffeine or caffeidine; and, in fact, Strecker found some other bodies formed which he has not yet investigated. In this transformation, caffeine, in being converted into caffeidine, exchanges CO for H^2 , and that in this

* Phil. Mag. vol. xvi. p. 138.

† Liebig's *Annalen*, September 1862.

‡ Ibid.

case the feebly basic properties of caffeine are increased by losing the acid radical $\text{C}\Theta$ agrees with other observations.

By the action of ammonia on chloracetic acid, glyocol, $\text{C}^2\text{H}^5\text{N}\Theta^2$, is obtained; by substituting in this reaction methylamine, $\text{C}\text{H}^5\text{N}$, for ammonia, Volhard* has found that sarcosine, $\text{C}^3\text{H}^7\text{N}\Theta^2$, is formed, the base which Liebig obtained as a product of the decomposition of kreatine.

Chloracetic ether was mixed with an excess of concentrated aqueous solution of methylamine, and the mixture heated in closed tubes to 125° . It was then boiled with baryta water until all methylamine was removed, the baryta separated by sulphuric acid, and the filtrate therefrom evaporated to dryness on the water-bath. From this mass, on recrystallization, pure hydrochlorate of sarcosine was obtained.

From this body the free base and its platinum-salt were procured.

The constitution of essential oil of rue has been found by Williams† to be $\text{C}^{22}\text{H}^{22}\text{O}^2$, which has been confirmed by Hallwachs‡. The former chemist considered it to be the aldehyde of a radical enodyle, $\left. \begin{array}{c} \text{C}^{20}\text{H}^{21}\text{O}^2 \\ \text{H} \end{array} \right\}$. Harbordt, who has recently§ examined this body, by analyses and determinations of the vapour-density has arrived at the same results in reference to its empirical formula; but he considers it to be differently constituted. The characteristic properties of aldehydes are that they combine with ammonia, and that by oxidation they yield an acid with the same number of atoms of carbon. The property of combining with alkaline bisulphites is common to both aldehydes and ketones. The chief constituent of oil of rue does, as Strecker has found, when saturated with ammonia and cooled, yield a crystalline body; but this only contains $\frac{1}{2}$ per cent. of ammonia, and is nothing more than unchanged oil. Harbordt has found that the oil of rue, when oxidized, even by the most gentle action, always yielded capric acid, $\text{C}^{20}\text{H}^{20}\text{O}^4$, and never an acid of the formula $\text{C}^{22}\text{H}^{22}\text{O}^4$, as it ought to have done had it been the corresponding aldehyde. He accordingly considers it to be a ketone, methylcaprinol, $\left. \begin{array}{c} \text{C}^{20}\text{H}^{19}\text{O}^2 \\ \text{C}^2\text{H}^3 \end{array} \right\}$.

Barth and Hlasiwetz|| have obtained a new acid from milk-sugar by the action of bromine upon it in the presence of water.

* Liebig's *Annalen*, August 1862. † Phil. Mag. S. 4. vol. xvi. p. 380.

‡ Liebig's *Annalen*, vol. cxiii. p. 107.

§ Ibid. September 1862.

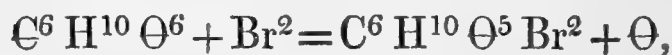
|| Ibid. April 1862.

Phil. Mag. S. 4. No. 164. *Suppl.* Vol. 24.

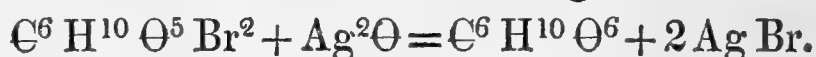
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30 grammes of milk-sugar and 60 of bromine were enclosed in a champagne-bottle with $\frac{1}{2}$ a litre of water; the bottle was closed by a caoutchouc stopper which was wired down, and the whole heated in boiling water until the mixture was colourless. The product was treated with oxide of silver, the filtrate treated with sulphuretted hydrogen, and the filtrate from this converted into the cadmium salt, which crystallizes in small masses. From this salt the acid was obtained by treatment with sulphuretted hydrogen. When evaporated to a syrup, it crystallizes in groups of needles, which, dried over sulphuric acid, are pulverulent. The acid is highly hygroscopic; it is soluble in alcohol, but not in ether; heated on platinum, it burns with the smell of sugar. It reduces silver-solutions and alkaline solutions of copper. The formula of the acid is $C^6 H^{10} O^6$.

The ammonia-salt, $C^6 H^9 NH^4 O^6 + 3 H^2 O$, and the soda-salt, $C^6 H^9 Na O^6 + 3 H^2 O$, crystallize well; the potash-salt is highly deliquescent. With the exception of a lead-salt there are no other salts than those with 1 equiv. of base, although it is very improbable, from the large quantity of oxygen it contains, that it is monobasic. It has the same formula as Wurtz's diglycol-ethylenic acid*, and for the present the authors propose for it the name *Isodiglycolethylenic acid*. It is probable that in its formation milk-sugar takes up bromine,



just as Kekulé found† that fumaric acid combined directly with bromine: this bromine compound then gives with oxide of silver the acid, in accordance with the following reaction:



Schwanert‡ has investigated camphrene, first obtained by Delalande by the action of sulphuric acid on camphor, and subsequently examined by Chautard. It is best prepared by heating camphor with four parts of sulphuric acid for five or six hours at a temperature of 100° . If the heating is not so long continued, much camphor remains unchanged; if longer, a profound decomposition is set up.

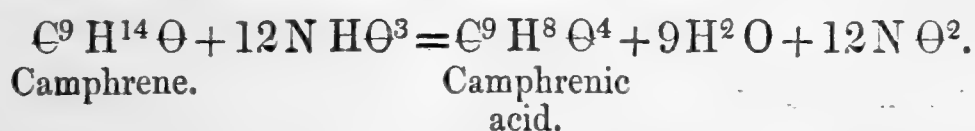
The product of this reaction is mixed with a large quantity of water, by which an oil is separated which swims on the surface. This is camphrene; but it contains a large quantity of undecomposed camphor. It is freed from this first by rectification, in which process an oil goes over that almost solidifies from the quantity of camphor it contains. The distillate between 220° and 240° contains less camphor; it is purified by heating it in a

* Phil. Mag. S. 4. vol. xx. p. 293.

† Ibid. vol. xxii. p. 306.

‡ Liebig's *Annalen*, September 1862.

current of hydrogen to near its boiling-point until no more camphor sublimes, and then rectifying. When pure, camphrene is a yellowish limpid liquid, of a pleasant aromatic odour and sharp taste. Its specific gravity is 0.9014, and it boils at 230° to 235° C. Its formula is $\text{C}^9\text{H}^{14}\Theta$, and is the same as that of phorone*, as is also the vapour-density. But the two substances are only isomeric, as Schwanert has shown by comparing the action of different agents on them. When phorone is treated with anhydrous phosphoric acid, the hydrocarbon cumole, C^9H^{12} , is produced; by the same treatment camphrene also yields a hydrocarbon of the composition of cumole, but boiling at 30° lower, and therefore not identical with it. By the action of nitric acid phorone is converted into a resin; camphrene is also acted on by nitric acid, and yields a resin which contains a new bibasic acid, *camphrenic acid*, $\text{C}^9\text{H}^8\Theta^4$, the formation of which may be thus expressed:



The salts of this acid are amorphous.

By the action of pentachloride of phosphorus on camphrene, a chloride of camphryle, $\text{C}^9\text{H}^{13}\text{Cl}$, is obtained. It is a colourless, transparent, refringent limpid liquid, with a pleasant aromatic smell, and a somewhat bitter sharp taste. It differs from chloride of phoryle produced by the action of pentachloride of phosphorus on phorone, in its higher boiling-point and its greater specific gravity.

By placing sodium in camphrene mixed with pure benzole until no further action took place, and then adding iodide of methyle, distilling, and repeatedly pouring back the distillate on the residue in the retort, a body, *methylcamphrene*, was obtained, which after appropriate purification constitutes a pleasant, ethereal, aromatic, limpid liquid, the formula of which is $\text{C}^9\text{H}^{13}(\text{C H}^3\Theta)$. *Acetylcamphrene* was similarly obtained.

LXXII. Correlations of Analysis.

By JAMES COCKLE, M.A., F.R.A.S., F.C.P.S. &c.†

1. **D**ENOTING differentiation by an accent, so that

$$\phi'(\alpha, \alpha_2, \dots \alpha_r; y) = \frac{d}{dx} \phi(\alpha, \alpha_2, \dots \alpha_r; y),$$

let us define

$$\phi(\alpha, \alpha_2, \dots \alpha_r; y)$$

* Phil. Mag. S. 4. vol. xiii. p. 188.

† Communicated by the Author.

by equating it with

$$\left\{ \frac{d}{dx} - \frac{\phi'(\alpha, \alpha_2, \dots, \alpha_{r-1}; \alpha_r)}{\phi(\alpha, \alpha_2, \dots, \alpha_{r-1}; \alpha_r)} \right\} \phi(\alpha, \alpha_2, \dots, \alpha_{r-1}; y),$$

so that, replacing $\phi(\alpha_0; \alpha)$ by α , we have

$$\left\{ \frac{d}{dx} - \frac{\alpha'}{\alpha} \right\} y = \phi(\alpha; y), \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$\left\{ \frac{d}{dx} - \frac{\phi'(\alpha; \alpha_2)}{\phi(\alpha; \alpha_2)} \right\} \phi(\alpha; y) = \phi(\alpha, \alpha_2; y), \quad . \quad . \quad . \quad (2)$$

$$\left\{ \frac{d}{dx} - \frac{\phi'(\alpha, \alpha_2; \alpha_3)}{\phi(\alpha, \alpha_2; \alpha_3)} \right\} \phi(\alpha, \alpha_2; y) = \phi(\alpha, \alpha_2, \alpha_3; y), \quad . \quad (3)$$

and so on. Then

$$\phi(\alpha, \alpha_2, \dots, \alpha_n; y) = 0 \quad . \quad . \quad . \quad . \quad . \quad (4)$$

is satisfied by

$$y = \alpha, \quad y = \alpha_2, \dots, y = \alpha_n;$$

and, if $\alpha, \alpha_2, \dots, \alpha_n$ be n particular integrals of a linear differential equation of the n th order, the development of (4) will give a symbolical decomposition of the differential equation analogous to the decomposition of an algebraical equation into linear factors.

The symbolical decompositions of a linear differential equation are, in general, infinite in number. If π denote the "symmetric product," and θ the "resolvent product," there are decompositions of the differential resolvents of cubics, biquadratics, and quintics which may respectively be represented by

$$\phi\left(A\alpha, \frac{B\pi}{\alpha}; y\right) = 0,$$

$$\phi\left(A\alpha, B\beta, \frac{C\pi}{\alpha\beta}; y\right) = 0,$$

$$\phi\left(A\alpha, B\beta, C\gamma, \frac{D\theta}{\alpha\beta\gamma}; y\right) = 0,$$

α, β, γ being constituents of the roots, and A, B, C, D arbitrary constants.

2. There are critical functions of the coefficients of differential equations analogous to those "critical functions" of the theory of algebraic equations which, in the theory of quantics, are termed leading coefficients of covariants, peninvariants, or seminvariants. Let uv be substituted for y in the linear differential equation

$$(1, b, c, d, \dots, \chi \frac{d}{dx}, 1)^n y = 0, \quad . \quad . \quad . \quad . \quad (a)$$

and let the result, divided by u , be

$$(1, \beta, \gamma, \delta, \dots \chi \frac{d}{dx}, 1)^n v = 0; \quad \dots \dots \dots (b)$$

then we have, by the aid of Leibnitz's theorem,

$$(1, b \chi \frac{d}{dx}, 1)u = \beta u, \quad \dots \dots \dots (5)$$

$$(1, b, c \chi \frac{d}{dx}, 1)^2 u = \gamma u, \quad \dots \dots \dots (6)$$

$$(1, b, c, d \chi \frac{d}{dx}, 1)^3 u = \delta u, \quad \dots \dots \dots (7)$$

and so on. Developing (5), (6), (7), we have

$$u' + bu = \beta u, \quad \dots \dots \dots (8)$$

$$u'' + 2bu' + cu = \gamma u, \quad \dots \dots \dots (9)$$

$$u''' + 3bu'' + cu' + du = \delta u. \quad \dots \dots \dots (10)$$

Differentiate (8), and by means of the result, combined with (8) and (9), eliminate u'' and u' . We find, on rejecting the factor u ,

$$b^2 + b' - c = \beta^2 + \beta' - c. \quad \dots \dots \dots (11)$$

Again, differentiate (8) twice and subtract the result from (10). In the difference substitute for u'' its value obtained from (9), and, by means of (11), eliminate β' . We find, neglecting the factor u ,

$$2b^3 - 3bc + d - b'' = 2\beta^3 - 3\beta\gamma + \delta - \beta''. \quad \dots \dots (12)$$

Hence the critical differential functions

$$b^2 - c + b', \\ 2b^3 - 3bc + d - b'',$$

remain unaltered by the above transformation from y to v .

In the algebraic equation

$$(1, b, c, d, \dots \chi y, 1)^n = 0, \quad \dots \dots \dots (a)$$

which yields (a) when exponents are changed into differential coefficients, let $v + u$ be substituted for y , and let the result be

$$(1, \alpha, \beta, \gamma, \delta \dots \chi v, 1)_n = 0; \quad \dots \dots \dots (b)$$

then

$$(1, b \chi u, 1) = \beta, \quad \dots \dots \dots (13)$$

$$(1, b, c \chi u, 1)^2 = \gamma, \quad \dots \dots \dots (14)$$

$$(1, b, c, d \chi u, 1)^3 = \delta, \quad \dots \dots \dots (15)$$

and so on. Developing (13), (14), (15), we have

$$u + b = \beta, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (16)$$

$$u^2 + 2bu + c = \gamma, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (17)$$

$$u^3 + 3bu^2 + 3cu + d = \delta; \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (18)$$

and, eliminating u between (16) and (17),

$$b^2 - c = \beta^2 - \gamma. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (19)$$

Again, multiplying (16) into u^2 , subtracting the product from (18), and eliminating u^2 and u from the difference by means of (17) and (16) respectively, we find, by the aid of (19),

$$2b^3 - 3bc + d = 2\beta^3 - 3\beta\gamma + \delta; \quad . \quad . \quad . \quad (20)$$

and indeed the forms of the two systems of equations indicate that, if we suppose $b, c, \dots \beta, \gamma$, &c. to be constant, the critical differential becomes the critical algebraical function.

3. In the last February Number I deduced, from one symbolical decomposition, the trigonometrical and (through logarithmic forms) the algebraical solution of a cubic. This may suggest the inquiry whether, by modifying the decomposition of the differential resolvent, we may not pass in other cases (and even where every solution is transcendental) from a more to a less restricted form of solution*.

Goldsmith Building, Temple, London,
November 8, 1862.

LXXIII. *On some Meteorites in the British Museum, &c.*

By R. P. GREG, Esq., F.G.S.†

IN consequence of a recent article in Poggendorff's *Annalen*, No. cxvi. p. 637, by Dr. Otto Buchner, of Giessen, comparing the national collections of meteorites at Vienna and London, I am reminded of the desirability of publishing a few details concerning some of the specimens in the British Museum, with which I happen to have had to do at various times. Dr. Buchner only takes into consideration the relative contents of the two collections at the beginning of last year; and if even on those data we admit that he has quite impartially decided in favour of the superior merits of the Vienna collection, and is

* I take this opportunity of referring to a paper "On the Theory of the Transcendental Solution of Algebraic Equations," by the Rev. Robert Harley, F.R.A.S. &c., printed at pp. 337-360 of the current 'Quarterly Journal of Pure and Applied Mathematics' (No. 20, October 1862, vol. v.), and also to a paper "On the Theory of Quintics, Part II.," by the same mathematician, which appears at pp. 248-260 of the same volume of that Journal (see No. 19, June 1862).

† Communicated by the Author.

right in looking on the total weight of the specimens in the British Museum as more than counterbalanced by the superior quality and greater number of stone-falls represented at Vienna, yet this would in no way derogate from the credit due, for his energetic administration, to the keeper of the mineral department in our National Museum, Mr. Nevil S. Maskelyne, who, during the very brief period he has been in office, has at least trebled the collection in every sense of the word. Indeed, since the lists were published from which Dr. Buchner drew the *data* for his criticisms, the progress of our national collection has been such as to prove that it may ere long be beyond the reach of rivalry in all that makes such a collection interesting and valuable*. It must, however, be borne in mind that it will be necessary to make the most of, and to display to the best advantage, as is done at Vienna, every single specimen. It is perhaps of greater importance to have 150 falls and localities of good average size, well selected and all characteristic, than even 200 which in form, in size, as well as in condition are not adapted to illustrate their characteristic features.

Nothing is more desirable than to have good standard specimens for purposes of comparison and study; in these they are doubtless richly provided at Vienna; but it must be borne in mind that there a steady and active attention has for many years been devoted to the formation and development of that fine collection of meteorites, and that almost every specimen has been more or less amply described in printed Catalogues and in Scientific Journals. The British Museum, on the other hand, possesses now a splendid material, and needs only the attention of its present efficient administration to be given to this point, to make it all that can be desired by scientific men. Indeed this matter has already been taken in hand by Mr. Maskelyne. It is not to be desired that the two collections to which we have been adverting should be stereotyped *fac-similes* of each other: each one already has its own peculiar types of excellence and interest; if the Vienna collection possesses more stone-falls than does the British Museum, the latter is richer probably in its irons; the

* Dr. Buchner in his paper states that there were last year, on a comparison of the respective catalogues, at Vienna 34 stones and 9 irons (=43 localities), which were then not represented in London; these 43 *desiderata* are now reduced to 25. Of the 26 localities in London (of which, however, probably at least 4 are doubtful) then unrepresented at Vienna, we believe 4 have since been received, thus giving 18 good localities in London that are wanting at Vienna, against 25 at Vienna that are not to be seen in London. Since these catalogues were published a large number of new specimens have been added to both collections; and at Vienna there are now we believe 120 stones and 68 irons=188; and Mr. Maskelyne informs me there are at the British Museum 79 irons and 111 stones=190.

former again is unsurpassed in its *suite* of meteorites from the Austrian dominions, the latter in those from India. In each collection, moreover, are to be found many *desiderata* which the other can supply; and it is to be hoped, for the sake of science, that a generous system of exchange may be adopted, and that a friendly rivalry may be the means of perfecting with mutual satisfaction the splendour and utility of these two great collections.

STONES.

1. NULLES.—An account of the fall of this meteorite is given in a curious pamphlet entitled *Lithologia Meteorica*, written by Dr. D. Joaquin Balcells, Professor of Natural Philosophy of the Industrial School at Barcelona, and published at Barcelona in the year 1854. The fall of a great number of stones occurred between the villages of Nulles, Vilabella, and Tarragona, some 15 or 16 leagues S.E. of Barcelona, on the evening of the 5th of November, 1851, the sky being at the time free from clouds. A fireball was first seen high up in the east, which seemed to take a S.W. direction, leaving behind it a luminous tail, which changed into a kind of mist that even remained visible during a period of 20 minutes; after the disappearance of the fireball a tremendous noise like thunder was heard. The meteor was seen by some at a distance of 20 leagues. One fragment, weighing 19 lbs. 8 ozs., was secured by the authorities and sent to Madrid; Dr. Balcells himself was fortunate enough to secure two stones, one weighing 690 grammes, of the form of a flattish triangular pyramid, and not entirely covered with black crust; the other fragment weighed 105 grammes; this was a fragment with three sides, showing no crust, merely fractured surfaces, resembling a greyish-blue sandstone containing metallic points. Dr. Balcells afterwards saw other fragments and entire stones presenting very similar characteristics.

The specific gravity was 3·46 according to Dr. Balcells, which is probably too low; Professor Escosura, Professor of Chemistry in the College of Mines at Madrid, states the specific gravity to be 3·818, and in chemical constitution that it much resembles the Blansko stone analysed by Berzelius. Professor Escosura found 100 parts separated by the magnet to contain

Iron	90·51
Nickel	5·68
Insoluble residue . . .	3·12
	<hr/>
	99·31

The non-magnetic portion contained, in 100 parts, respectively—

	Soluble in acids.	Insoluble.
Silica	33·05	57·72
Magnesia	32·07	21·40
Oxide of iron	24·10	15·51
Alumina	1·04	2·16
Sulphuret of iron	8·70	
Oxide of manganese		0·60
Chromic iron		1·30
	<u>98·96</u>	<u>98·69</u>

The entire composition of this *aërolite* may therefore be represented as follows:—

Alloy of iron and nickel	$\left\{ \begin{array}{l} \text{Iron} \quad . \quad . \quad 22\cdot50 \\ \text{Nickel} \quad . \quad . \quad 1\cdot43 \\ \text{Residue} \quad . \quad . \quad 0\cdot79 \end{array} \right\} 24\cdot72$
Soluble in acids	$\left\{ \begin{array}{l} \text{Silica} \quad . \quad . \quad . \quad 10\cdot00 \\ \text{Magnesia} \quad . \quad . \quad . \quad 9\cdot67 \\ \text{Oxide of iron} \quad . \quad . \quad 6\cdot45 \\ \text{Alumina} \quad . \quad . \quad . \quad 0\cdot31 \\ \text{Sulphuret of iron} \quad 2\cdot34 \text{ FeS} \end{array} \right\} \left. \begin{array}{l} 3\text{RO} + \text{SiO}^3 \\ \\ \\ \\ \end{array} \right\} 28\cdot77$
Insoluble in acids	$\left\{ \begin{array}{l} \text{Silica} \quad . \quad . \quad . \quad 26\cdot43 \\ \text{Magnesia} \quad . \quad . \quad . \quad 9\cdot80 \\ \text{Oxide of iron} \quad . \quad . \quad 7\cdot10 \\ \text{Oxide of manganese} \quad 0\cdot30 \\ \text{Alumina} \quad . \quad . \quad . \quad 0\cdot53 \\ \text{Chromic iron} \quad . \quad . \quad 0\cdot59 \end{array} \right\} \left. \begin{array}{l} 3\text{RO} + 2\text{SiO}^3 \\ \\ \\ \\ \text{FeO} + \text{Cr}^2 \text{O}^3 \end{array} \right\} 44\cdot71$
	<u>98·20</u>

The composition of the *soluble* portion, Professor Escosura remarks, corresponds to the formula of the tribasic silicates, $3\text{RO} + \text{SiO}^3$, which is analogous to the variety of olivine called *hyalosiderite*.

The *insoluble* portion can be represented by the formula of the sesquisilicates, $3\text{RO} + 2\text{SiO}^3$, which may best be referred to some varieties of augites (probably *anthophyllite*).

The small specimen of the Nulles *aërolite* in the British Museum is a portion of that sent me last year by Professor Balcells himself, and is remarkable for the unusually large quantity as well as size of the metallic particles it contains; in this respect it resembles the Macao stone, and, like most specimens of that fall, is of a dark reddish-brown colour—so much so that, were it not for Dr. Balcells stating that the fresh fracture originally presented the appearance of a greyish-blue sandstone, one would have considered that it actually formed the connecting link between that stone and the meteorite of Hainholz. Its hardness

is considerable, and it is capable of receiving, after being cut, a high degree of polish.

2. *DURUMA*.—A portion of a stone, weighing somewhat over 1 lb., partially coated with a blackish-brown crust, is deposited in the mineralogical collection in the Museum of the University of Munich: the accompanying label merely states that it fell on the 6th of March, 1853, in Duruma, Wanika-land, East Africa. Nothing more appears to be known, even at Vienna, or by Dr. Buchner of Giessen, respecting this stone. Ascertaining from the MS. Catalogue that this meteorite had been presented by a Dr. Barth of Calw in Würtemberg, I wrote at once to that gentleman, and received the following reply:—"The meteoric stone fell during a violent thunder-storm in the district of Duruma, in the Wanika country in 1853, and was at once anointed with oil by the natives, dressed with beads, and set up as a god.

"In vain did the Missionary, the Rev. Mr. Rebmann, try to procure it for the collection of Dr. Barth, until some years afterwards, when the wild Masai conquered the country, killing the inhabitants and burning their dwellings; then the Wanikas felt themselves abandoned by their god; a famine also occurred about the same time; so the stone was given up to Mr. Rebmann, and sent to Dr. Barth." From Dr. Kurr of Strasbourg, who made a qualitative analysis (it contained olivine, lime, hydrate of iron, oxide of iron, and nickeliferous iron), I received what remained over from the analysis, a few crumbs weighing only $1\frac{3}{8}$ gramme; a portion of which I presented as a curiosity to the British Museum. The stone, judging by the large fragment at Munich, appears to contain a good deal of nickeliferous iron, and is rather coarse-grained, is more or less oxidized, and much resembles the Segowlee stone in the British Museum, said to have fallen in India the very same day.

3. *GIRGENTI*.—The specimen in the British Museum (formerly belonging to the Allan-Greg collection) is a portion of a specimen given me when in Sicily in the year 1855, by Professor Gemmellaro of Catania. The specimen in his collection was also a fragment, weighing probably about 2 lbs., and much coated with crust; this meteorite fell on the 10th of February 1853; its specific gravity is 3.76, and it almost exactly resembles the meteoric stones of Bachmut, New Concord, Château-Renard. I have seen no published account of this stone; but, if I recollect correctly, Professor Gemmellaro informed me an account had been printed in a Sicilian scientific journal.

4. *EKATHERINOSLAW*.—In the mineralogical collection of the late Mr. Allan of Edinburgh, and now deposited in the British Museum, is a specimen referred to in the MS. Catalogue as having formed a portion of a stone weighing 85 lbs. which fell

in the government of Ekatherinoslaw, 1825,—the principal mass being in the (?) Museum of Odessa. Now as no very precise date is given, and there is a well-authenticated account of a stone of 80 lbs. having fallen near Paulograd in Ekatherinoslaw on the 19th of May 1826, it is very reasonable to suppose, knowing how in such cases mistakes are not of unfrequent occurrence, that 1825 is a mistake for 1826. Dr. Hörnes and M. Haidinger of Vienna are of opinion, however, that this specimen in the British Museum in all probability is identical with the meteoric stone of Bachmut, also a city in the government of Ekatherinoslaw, which fell on the 3rd of February 1814; or rather two stones are said to have fallen on that occasion, one of 40 lbs., the other of 20 lbs.; and they are of this opinion from several reasons: *first*, that the stone in question greatly resembles the Bachmut stone; *secondly*, that there are several collections which contain genuine specimens of Bachmut, whereas undoubted specimens of the fall of May 19th, 1826, are unknown at present; *thirdly*, that no specimen either of Bachmut or Paulograd can now be found at Odessa. These reasons are not, however, by any means conclusive. Dr. Buchner of Giessen informs me that such a stone was seen by a scientific traveller at Odessa in the year 1829, which was about the time when Mr. Allan's MS. Catalogue was written; and it is very possible, as Paulograd is on the high road to Odessa, that this stone may have been sent to that city; it is clear, however, that no specimens went to St. Petersburg, whither specimens of Bachmut found their way on an earlier occasion. It is far more probable that 1825 was a mistake for 1826 than for 1814; and it is quite as probable that specimens went to Odessa as that specimens of the 1814 fall at Bachmut (about 100 miles distant) found their way to St. Petersburg and Vienna. The physical appearance of the stone in question certainly much resembles that of Bachmut; but so do a considerable number of others: there appears to be rather a greater tendency to oxidation of the metallic particles in the Bachmut stone. I may add that the specimen in question was presented to Mr. Allan by a Dr. Dowler.

5. LONTALAX.—The specimen of Lontalax in the British Museum is certainly incorrectly named, and is in all probability Timochin, 13th March 1807. It is an old specimen, having been acquired by the late Mr. Koenig. Dr. Buchner says the true spelling of *Lontalax* is Luotlaks, a place near Wiborg in Finland. The genuine Luotlaks stone (13 Dec. 1813) is a very peculiar one, and, from its trachytic nature, paucity of metallic iron, and imbedded crystals of olivine, belongs to the group of rare meteorites embracing those known in the catalogues as Bialystock, Mässing, Nobleborough, Managaon, and St. Petersburg.

6. PLESCOWITZ, Bohemia, 22 June 1723.—This specimen, also a unique one if correctly labelled, is deposited among the meteorites in the British Museum. There is strong reason for supposing this is really a portion of the Tabor, 3rd July 1753, meteorite; the physical characteristics are nearly the same, both belonging to the *bluish* meteorites of Reichenbach (see Note). The Plescowitz is a very ancient and rather obscure fall; no specimens are known to be preserved in Europe, while Tabor is one far better known, and of which specimens are not uncommon.

7. MARMANDE, Lot-et-Garonne, France.—This is represented by a small and unique specimen in the British Museum; it was purchased by myself last year from the late M. Boubée, a geologist and mineral-dealer of Paris; the specimen is without any crust, and is accompanied by a label stating it to have been part of a stone which fell near Marmande, July 4th, 1848; M. Boubée knew nothing more about it, than that he had found it, as well as the label, among specimens purchased as part of the effects of a deceased Colonel in the French army. The stone resembles in general colour and appearance stones of the Château-Renard class, but on a closer examination shows a more crystalline texture, and may be said to come between that stone and the peculiar meteorite of Juvenas.

There appears to be no published account of this fall, nor are there any other specimens known in any of the English or Continental collections. On the whole, there does not seem to be any strong reason for not considering this otherwise than as an authentic specimen.

IRONS.

8. LIVINGSTON County, Kentucky.—This iron was originally described in 1846, by the late Dr. Troost, in whose collection is probably to be found the principal mass. Specimens of this iron are very rare. In a qualitative analysis lately made for me by Professor Roscoe, of Owens College, Manchester, an unusually large proportion of nickel was detected. This iron, when etched with acids on a polished surface, shows no Widdmanstätten figures whatever, or even semblance of crystalline structure, merely some small bright spots closely scattered at pretty regular intervals; in both these respects it resembles the iron of Green County, Tennessee, and to some extent that of Rasgata.

9. DICKSON County, Tennessee; fell 31st August, 1835; described by Dr. Troost in 1845, in Silliman's Journal.—The principal mass is in Dr. Troost's collection, and weighs about 9 lbs.; it is pear-shaped, and outside crust quite bright, without any oxidation. Specimens of this iron are very rare; there is a small specimen in the British Museum, but unetched; a small speci-

men in my own collection shows, when etched, on a small scale the beautiful crystalline figures of the Toluca iron.

10. SCRIBA, Oswego County, New York, described by Professor Shepard in 1841 (Sill. Journal, vol. xl. p. 366).—No proper analysis appears to have been made of this iron; and latterly it has been placed among the doubtful or pseudo-meteorites at Vienna, as well as in Professor Shepard's private collection. A qualitative analysis recently made of this iron by my friend Professor Heddle of St. Andrews, Fife, showed that it contained—

Iron.	Silicon	} Traces.
Nickel.	Sulphur	
Aluminium.	Carbon	
Calcium.	Phosphorus?	
Potassium (? potash).	Tin?	
Sodium, traces.		

There were no traces of either cobalt, chromium, or manganese.

This iron possesses no regular crystalline structure; but nevertheless when etched it shows a peculiar *moire antique* surface, with a finely scaly or nacreous structure. In this it curiously resembles the Otumpa iron, of which there is so characteristic and well-prepared a specimen in the collection at Vienna. The specimen of the latter in the British Museum requires being cut and polished to show these characteristics.

There are several other meteoric specimens in the British Museum, of which Mr. Maskelyne is publishing particulars. Of newly discovered or described meteoric stones there are two, neither of which are as yet represented in collections in this country, but to which I may very briefly allude in this place.

First. A specimen deposited in the Museum at St. Petersburg, from the northern side of the Caucasian Mountains: this stone fell near Stauropal, 26th March 1857. I believe some particulars of the fall have been published in one of the St. Petersburg scientific journals.

Secondly. A meteoric stone of some 21 lbs. fell at Meno near Fürstenberg in Mecklenburg, at 12½ p.m., with a tremendous noise, on the 7th October last: it was coated with crust, contains iron, the sp. gr. being about 4.0. It fell into some sand, close to the feet of a peasant.

This is the first mention of any meteoric fall this year, the usual average being seven or eight falls.

Meteoric showers.—The usual displays of shooting stars this year, as seen near Manchester, have not been as well marked as usual; that of August 10–11 perhaps less so than those for the last few years, but the weather was not very favourable; that of November 9–10 was not in the least marked, either as regards

numbers or radiant; but the more newly determined period for December 10–12 has been exceedingly well-defined, and the radiant point, both for last year and for the present one, perfectly referable to a part of the heavens halfway between β Auriga and α Gemini.

Manchester, 12th December.

Note on Bluish Meteorites.

There seems good reason for believing that the class of *bluish* meteorites, of which the Barbotan stone may be considered the type, differs from the *whitish* meteorites, represented by the Château-Renard stone, chiefly by reason of the *stony* portion containing a smaller proportion of soluble silicate in the shape of olivine, and a larger proportion of minerals of the augitic and felspathic class, which are insoluble in acids. There is also more lime in the Barbotan stone than in the Château-Renard stone; and this lime evidently belongs to the felspathic element, rather than to the olivine. Perhaps the amount of lime and alumina contained in a meteoric stone may fairly be considered as an index or exponent of the relative quantities of these two minerals present, and especially of the felspar as compared with the olivine. We have, moreover, a regular gradation, as it were, in meteorites—that is, as regards their non-magnetic constituents—from the Chassigny stone, almost entirely composed of olivine, through mixed stones of the Château-Renard and Barbotan class (containing also an admixture of particles of nickeliferous iron), up to stones of the Juvenas and Stannern class, which contain no olivine, but consist principally of augite (anthophyllite) and felspar (anorthite), with no metallic iron.

LXXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 491.]

Feb. 13, 1862. “ON Magnetic Calms and Earth-Currents.” By Charles V. Walker, Esq., F.R.S., F.R.A.S. &c.

Feb. 20.—“On the Dicynodont Reptilia, with a Description of some Fossil Remains brought by H.R.H. Prince Alfred from South Africa, in November 1860.” By Professor R. Owen, F.R.S. &c.

February 27.—Major-General Sabine, President, in the Chair.

The following communications were read:—

“Notices of some Conclusions derived from the Photographic Records of the Kew Declinometer, in the years 1858, 1859, 1860, and 1861.” By Major-General Edward Sabine, P.R.S.

The discussion of the magnetic observations which have been made in different parts of the globe may now be considered to have

established the three following important conclusions in regard to the magnetic disturbances : viz., 1. That these phenomena, whether of the declination, inclination, or total force, are subject in their mean effects to periodical laws, which determine their relative frequency and amount at different hours of the day and night. 2. That the disturbances which occasion westerly and those which occasion easterly deflections of the compass-needle, those which increase and those which decrease the inclination, and those which increase and those which decrease the magnetic force have all distinct and generally different periodical laws. 3. That there exists a periodical variation in the relative amount of disturbance in different years, constituting a cycle of about ten terrestrial years, which has been found to correspond, both in the duration of the period and in the epochs of maxima and minima, with a periodical variation in the appearance of spots on the solar disk.

In the introductions prefixed to the several volumes containing the observations made at the colonial observatories, the concurrent testimony of the disturbances of the three magnetic elements to these conclusions is fully exhibited; and in reference particularly to the third, viz. the decennial variation, a *résumé* has been given in the second St. Helena volume, pages cxxii to cxxxvi.

In that *résumé*, the particular form of the previously announced decennial variation is more fully traced, and, from the analysis of the observations, shown to be of the following character. If we begin with the part of the cycle to which the maximum of disturbance belongs, we find, first, three consecutive years in each of which the aggregate amount of disturbance (measured from a constant value) is nearly the same; then, two years of diminished disturbance; and then, three years in each of which the aggregate amount is nearly the same, but is considerably less than in the two preceding years, and very considerably less than in the three commencing years. The three years of minimum are then succeeded by two of medium disturbance, and these by the recommencement of three years of maximum amount. Thus, for example, referring to the years in which the colonial observatories were in action, 1841 and 1842 were years of medium disturbance; 1843, 1844, and 1845 years of minimum, differing little from each other; 1846 and 1847 years of medium, and 1848, 1849, and 1850 years of maximum. The general analogy of these particular features with Schwabe's observations of the solar spots, commenced in 1826 (showing, on the one hand, the number of groups of spots, and on the other hand, the number of days free from spots in each year), may be examined by a reference to the table in the third volume of 'Cosmos' (English translation), page 292, and is as satisfactory as, from the nature of the subject, could well be expected*.

The discontinuance of the colonial observatories occasioned a tem-

* The variation in the aggregate amount of disturbance in the years of minimum, 1843, 1844, and 1845, compared with the two years of medium immediately preceding, 1841 and 1842, and with the two years of medium immediately

porary suspension of investigations which are now admitted to have been of very high interest; but by the liberality and public spirit of the British Association, and by the aid of occasional grants of money from the Royal Society, apparatus for their resumption was completed at the Kew Observatory in 1857, and the investigations were recommenced on the 1st of January, 1858. The results obtained from the photographic records of the Kew declinometer in 1858 and 1859, with a full description of the methods and processes employed in their elicitation, were communicated to the Royal Society in 1860, and are printed in vol. xxii. of the *Phil. Mag.* pp. 310–324. The two years which have since elapsed have furnished similar results for the years 1860 and 1861, strictly comparable with those of 1858 and 1859, having been obtained with the same instruments and by the same methods. We have now, therefore, the observations of four consecutive years from the Kew Observatory, and we are thereby enabled to infer, by the comparison of the aggregate amount of disturbance in each of those years, the progression of the decennial variation up to the close of 1861. The aggregate amounts of disturbance in the four years were severally as follows:—

1858, January 1 to December 31,	7263·7 mins. of arc.
1859, " "	7637·3 "
1860, " "	7540·2 "
1861, " "	6461·6 "

The observations of preceding years had led to the expectation that 1858, 1859, and 1860 would be the three years of maximum, in which the aggregate amounts of disturbance would differ but little, and that 1861 would be the first year of medium, showing an aggregate amount of disturbance considerably below 1858, 1859, and 1860. This expectation has been realized; and we have now before us the prospect that the present year, 1862, will prove to be the second year of medium, with an aggregate amount of disturbance nearly resembling that in 1861, but a little less; and that 1863, 1864, and 1865 will be years of minimum, differing little from each other in the amount of disturbance, and all lower than the preceding years 1861 and 1862 on the one hand, or the succeeding years 1866 and

succeeding, 1846 and 1847, is well shown by the results of the hourly observations made in those years at the Hobarton Magnetic Observatory.

		Minutes of arc.
1841, Jan. 1 to Dec. 31; aggregate values	5441·9	} 4761·4.
1842, " " "	4080·8	
1843, " " "	2183·4	} 2565·2.
1844, " " "	2948·6	
1845, " " "	2563·7	} 4309·5.
1846, " " "	3735·5	
1847, " " "	4883·4	

The aggregate values which are here given are the amounts in each year of the disturbances exceeding 2'·13, reckoned from the normals of the several months and hours.

1867 on the other. Hence we see the importance of maintaining, during the remaining portion of the decennial period, the photographic records of the Kew Observatory, with as little change as may be practicable in the instruments and methods which have been employed during the first portion.

The Table which is printed in *Phil. Mag.* vol. xxii. p. 312, shows the aggregate values of the disturbances in 1858 and 1859 distributed into the several solar hours of their occurrence, and distinguishing between the disturbances which produce westerly and those which produce easterly deflections of the compass-needle. It also exhibits the ratios of disturbance at the several hours to the mean of the 24 hours taken as the unit. The subjoined Table contains the same particulars for the *four* years, 1858 to 1861, inclusive. It has of course a somewhat higher authority than the earlier table, inasmuch as ratios obtained from the records of four years are to be preferred to those derived from two years only. But the principal point of interest in comparing them with each other is the evidence which their correspondence affords, of the substantial truth of the two first of the three general conclusions adverted to in the commencement of the present communication, viz., the periodicity of the disturbances in respect to the several hours of solar time, and the distinct character of the laws which regulate the disturbances producing westerly deflections, and those producing easterly deflections. The principal features of both classes of disturbance are the same, whether viewed in the record of the two or of the four years. Regarded from either point of view, both classes follow progressions manifestly dependent upon the hours of solar time, the progressions of the westerly and those of the easterly deflections being as manifestly governed by distinct and different laws. The westerly deflections have their chief prevalence from 5 A.M. to 5 P.M., or during the hours of the day, the ratios at all the other hours being below unity. The easterly deflections, on the other hand, prevail chiefly during the hours of the night, the ratios being for the most part below unity at the hours when the westerly are above unity, and, conversely, above when the westerly are below. The easterly have one decided maximum at 11 P.M., towards which they steadily and continuously progress from 5 P.M., and from which they as steadily, and continuously, recede until 5 A.M. the following morning. The westerly appear in both records to have a double maximum, one about 6 or 7 A.M., the other about 2 or 3 P.M.

The main object of the Table is to exhibit the amounts of disturbance and the ratios at the several hours, derived from the photographic records of the four years; but, in order to facilitate the examination of the correspondence in these respects of the results severally deducible from the two and from the four years, the ratios of westerly and of easterly disturbance at the different hours which were derived from the photographic records in 1858 and 1859 are added, being reproduced from the Table, *Phil. Mag.* vol. xxii. p. 312.

TABLE showing the aggregate Values of the larger Disturbances of the Declination at the different hours of solar time in 1858, 1859, 1860 and 1861, derived from the Kew Photographic Records; with the Ratios of Disturbance at the several hours to the mean hourly value taken as the Unit.

Local astronomical time.	Westerly deflections in four years.		Easterly deflections in four years.		Ratios in two years.		Local civil time.
	Aggregate values.	Ratios.	Aggregate values.	Ratios.	Westerly.	Easterly.	
Hours.	Min. of arc.		Min. of arc.				Hours.
18	860.5	1.55	210.9	0.33	1.85	0.37	6 A.M.
19	904.9	1.63	221.1	0.34	1.83	0.38	7 A.M.
20	769.7	1.38	219.8	0.34	1.48	0.36	8 A.M.
21	732.1	1.32	234.3	0.36	1.23	0.38	9 A.M.
22	640.0	1.15	245.7	0.38	1.26	0.33	10 A.M.
23	696.3	1.25	228.9	0.35	1.21	0.39	11 A.M.
0	855.5	1.54	234.7	0.36	1.38	0.54	Noon.
1	947.0	1.70	218.3	0.34	1.44	0.48	1 P.M.
2	941.6	1.69	263.6	0.41	1.53	0.54	2 P.M.
3	954.5	1.72	197.5	0.30	1.71	0.34	3 P.M.
4	847.1	1.52	265.7	0.41	1.35	0.44	4 P.M.
5	595.1	1.07	332.6	0.51	1.15	0.51	5 P.M.
6	458.7	0.82	477.8	0.74	0.94	0.91	6 P.M.
7	272.0	0.49	798.6	1.23	0.37	1.19	7 P.M.
8	196.0	0.35	962.6	1.49	0.22	1.56	8 P.M.
9	230.9	0.42	1184.1	1.83	0.29	1.79	9 P.M.
10	148.6	0.27	1512.8	2.33	0.20	2.25	10 P.M.
11	121.9	0.22	1615.2	2.49	0.12	2.38	11 P.M.
12	266.5	0.48	1471.3	2.27	0.49	2.21	Midnight.
13	245.7	0.44	1352.7	2.09	0.47	1.98	1 A.M.
14	306.7	0.55	1291.9	1.99	0.49	1.80	2 A.M.
15	287.6	0.52	988.0	1.52	0.51	1.45	3 A.M.
16	407.1	0.73	702.7	1.08	0.97	0.95	4 A.M.
17	662.9	1.19	322.5	0.50	1.53	0.45	5 A.M.
Mean hourly values }	556.2 = 1.00		648.1 = 100				

In discussions published elsewhere the preponderance of westerly over easterly deflection, or the converse, has been inferred to be a *geographical characteristic* rather than an accidental feature. All the stations in North America, at which investigations have hitherto been made, concur in showing a considerable predominance of easterly deflections, whilst at Pekin in Northern Asia the converse is observable. Regarding Kew as the only representative station in the British Islands (the only one in which this investigation has been made), it is deserving of notice, that we find in this locality no constant or decided predominance of either class of disturbance over the other. There is indeed a slight preponderance of easterly values on the average of the four years, but not of such amount or regularity as to give it the character of a decided feature.

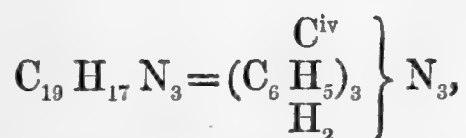
“On the Action of Chloride of Iodine on Iodide of Ethylene and Propylene Gas.” By Maxwell Simpson, M.B.

“Letter to the Council from Sir George Everest, C.B., on the Expediency of re-examining the Southern Portion of the Great Indian Arc of the Meridian; and Report of a Committee thereupon.”

March 6.—Major-General Sabine, President, in the Chair.

“On the Colouring Matters produced from Aniline.” By A. W. Hofmann, Ph.D, LL.D, F.R.S. &c.

In a note on the Action of Tetrachloride of Carbon on Aniline submitted to the Royal Society on the 17th of June, 1858, I have described a crystalline basic derivative of aniline formed by the coalescence of three molecules of ammonia, viz. carbotriphenyltriamine,



the formation of which is accompanied by that of a colouring matter of a magnificent crimson colour.

It may be useful to quote here the passage* of the paper referred to, in which the formation of the colouring matter is mentioned. “On submitting a mixture of $3\frac{1}{2}$ parts by weight of aniline and 1 part of bichloride of carbon, both in the anhydrous state, for about thirty hours to a temperature of 170°C ., the liquid will be found to be converted into a black mass, either soft and viscid, or hard and brittle, according to time and temperature.

“This black mass, which adheres firmly to the tubes in which the reaction has been accomplished, is a mixture of several bodies. On exhausting with water, a portion dissolves, while a more or less solid resin remains behind.

“The aqueous solution yields, on addition of potassa, an oily precipitate containing a considerable portion of unchanged aniline; on boiling this precipitate with dilute potassa in a retort, the aniline distils over, whilst a viscid oil remains behind, which gradually solidifies with a crystalline structure. Washing with cold alcohol and two or three crystallizations from boiling alcohol render this body perfectly white and pure, a very soluble substance of a magnificent crimson colour remaining in solution.

“The portion of the black mass which is insoluble in water dissolves almost entirely in dilute hydrochloric acid, from which it is reprecipitated by the alkalis in the form of an amorphous pink or dingy precipitate soluble in alcohol with a rich crimson colour. The greater portion of this body consists of the same colouring principle which accompanies the white crystalline substance.”

The action of tetrachloride of carbon on aniline yields only a comparatively small quantity of the crimson pigment; the temperature of the exposure, and the relative proportions in which the two substances act upon one another, have the greatest influence upon the results of the reaction. The white crystalline base, and the base dissolving with a crimson colour, are by no means the only products;

* Proceedings of the Royal Society, vol. ix. p. 294.

other bases, most of them amorphous and accessible only in the form of platinum-salts, are produced, and complicate, owing to the similarity of their chemical characters, the purification of the new compound. Notwithstanding many efforts, I failed in obtaining the new colouring matter in a state fit for analysis, and for the time abandoned the inquiry.

Industry, however, was not long in discovering new and much more appropriate methods for the production of the crimson aniline dye. Certain metallic chlorides (tetrachloride of tin) and nitrates (mercurous nitrate), and numerous oxidizing agents are capable of converting aniline into the crimson colouring matter. It was M. Verguin who first prepared this colour upon a large scale by the action of tetrachloride of tin on aniline. Since that time the production of the aniline-crimson has become an important industry, which, in the hands of Messrs. Simpson, Maule, and Nicholson in this country, of Messrs. Renard frères in France, has rapidly attained to colossal proportions. The interest attached to the subject is sufficiently evident by a glance at the periodical literature of the day. The journals of applied chemistry teem with the descriptions of processes for the production of the aniline-crimson, for which the names fuchsine, magenta, and others more fanciful have been proposed. Even the action of tetrachloride of carbon on aniline, little promising as it appeared at first, has been used upon the large scale; and interesting papers upon the industrial production of the colour by this process have been published by M. Charles Dolfus Galline*, by Messrs. Monnet et Dury†, and lastly by M. Lauth‡, who have proved that aniline-crimson, prepared upon the large scale by means of tetrachloride of carbon, may be applied in dyeing with exactly the same result as the colouring matter produced by other processes. It is not the object of this Note to enter into a detailed account of the development of this new industry, which has been admirably traced by M. E. Kopp in a series of interesting articles published in the 'Répertoire de Chimie Appliquée;' but I thought it right to quote the above authorities in order to show that the basic colouring matter obtained by me in 1858, while studying the action of tetrachloride of carbon upon aniline, is identical with the aniline-crimson which is now by various processes manufactured upon an enormous scale.

A substance possessing such remarkable properties as aniline-crimson, and accessible, moreover, as a commercial product, could not fail to attract the attention of scientific inquirers. The subject has been examined in succession by M. Guignet§, M. Béchamp||, M. Wilm¶, Messrs. Persoz, De Luynes et Salvétat**, M. Schneider††, and more recently by M. Emile Kopp‡‡ and M. Bolley§§. The conclusions, however, at which these experimentalists have arrived are far from concordant. I attribute this discrepancy in the results

* Répertoire de Chimie Appliquée, 1861, p. 11. † Ibid. p. 12.

‡ Ibid. p. 416. § Bull. Soc. Chim. Séance du 23 Déc. 1859.

|| Annales de Chim. et de Phys. [3] tome lix. p. 396.

¶ Bull. Soc. Chim. Séance du 27 Juillet 1861.

** Comptes Rendus, li. 538. †† Ibid. li. 1087.

‡‡ Annales de Chim. et de Phys. tome xii. p. 222.

§§ Dingler's Journal, clx. 57.

obtained by such able observers to the extreme difficulty of procuring the colouring matter in a state of purity, and to the circumstance that the slightest contamination with other bodies is capable of altogether masking the properties of this remarkable compound.

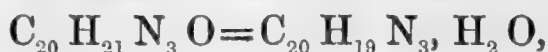
The red colouring matter of aniline and its saline compounds have been obtained for the first time in the state of purity by my friend and former pupil Mr. Edward Chambers Nicholson, a chemical manufacturer equally distinguished for his scientific attainments as for the skill and indomitable energy with which in many instances he has succeeded in rendering the results of purely scientific inquiries available for the purposes of life.

Mr. Nicholson has, with the utmost liberality, placed at my disposal not only a very considerable supply of the beautiful compounds which he produces, but also the vast and precise information which in his protracted experiments upon this subject he has accumulated. It is entirely through the kindness of Mr. Nicholson that I was enabled to resume the study of these remarkable bodies, a short account of the composition and of the chemical nature of which I beg leave to submit to the Royal Society.

Mr. Nicholson designates the pure base of the red colouring matter by the name of Roseine, which appears very appropriate, since this substance, which furnishes such splendidly coloured solutions, is absolutely white in the solid condition. Nevertheless, since the compound in question appears to be the prototype of a number of similar substances obtainable by similar processes from the homologues, and probably also from the analogues, of aniline, it may be useful to commemorate the origin of the compound in its name, and I accordingly propose the term *Rosaniline* for the designation of the new substance.

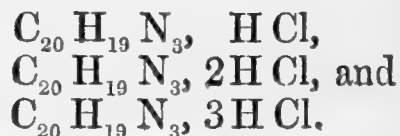
Rosaniline.—The material from which the base may be most conveniently obtained is the acetate which in practice is generally used for dyeing. This acetate Mr. Nicholson produces on the large scale in a state of perfect purity. The boiling solution of this salt, when supersaturated with a large excess of ammonia, furnishes a rose-red somewhat crystalline precipitate, which constitutes the base in a tolerably pure state. The colourless liquid filtered off while boiling from this precipitate deposits, on cooling, perfectly white needles and plates, which are the rosaniline in a state of absolute purity. Unfortunately the solubility of rosaniline in boiling ammonia, and even in boiling water, is extremely limited, so that only a very small proportion of the compound is obtained in the perfectly white condition. The base is somewhat more soluble in alcohol, the solution having a deep-red colour; it is insoluble in ether. Exposed to the action of the atmosphere, rosaniline turns rapidly pink, and ultimately dark red. No perceptible alteration of weight is observed during this change. At the temperature of 100° the base rapidly loses a minute quantity of water of interposition; it may then be heated to 130° without further losing weight. At a higher temperature rosaniline is decomposed with evolution of an oily liquid containing much aniline, a quantity of charcoal remaining behind.

The combustion of rosaniline has led to the formula



which has been corroborated by the examination of numerous well-characterized salts and derivatives.

Rosaniline is a well-defined powerful base, which forms several series of salts, nearly all remarkable for the facility and beauty with which they crystallize. The proportions in which this substance unites with acids characterize it as a triacid triamine. Like several other triamines which I have examined, it will probably be found to produce three classes of salts, viz.,

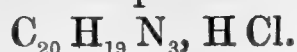


Up to the present moment, however, I have been able to obtain only representatives of the first and the third of these classes. The predilections of rosaniline are essentially monacid. The salts with one equivalent of acid are wonderfully stable compounds. I have recrystallized them four and five times without producing the slightest alteration in their composition. The salts with three equivalents of acid present comparatively little stability, being, in fact, decomposed by the action of water or by exposure to 100° .

By a glance at the formula given, it is obvious that the white crystals of the base itself, which were submitted to examination, are a hydrate,—the saline compounds of rosaniline, as might have been expected from many of the processes of their formation, containing no oxygen. The salts of rosaniline may be obtained by two different processes : either by the direct action of the respective acids, or by submitting the ammonium-compounds of the several acids to ebullition with an excess of the free base. Both processes yield the salts equally pure and of exactly the same composition. The salts with one equivalent of acid exhibit for the most part, in reflected light, the splendid metal-lustrous green of the wings of the rose-beetle ; in transmitted light the crystals are red, becoming opaque when they acquire certain dimensions. The solutions of these salts in water or alcohol possess the magnificent crimson colour for which rosaniline has become so justly celebrated. The salts with three equivalents of acid, on the other hand, are yellowish brown, both in the solid state and in solution. They are much more soluble in water and alcohol than the monacid salts, which for the most part are comparatively sparingly soluble. Both classes of rosaniline-salts crystallize readily, more especially the monacid compounds ; some of them Mr. Nicholson has obtained in perfectly well-formed crystals, which are at present in the hands of Quintino Sella for crystallometrical examination.

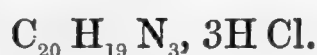
Chlorides.—These substances, and more especially the monacid salt, were of particular use in fixing the formula of rosaniline. Prepared either by the action of hydrochloric acid, or by means of chloride of ammonium, the salt is deposited from the boiling solution in well-defined rhombic plates, frequently united in stellar forms. The chloride is difficultly soluble in water, more soluble in alcohol, insoluble in ether.

The salt retains a minute quantity of water at 100°, but becomes anhydrous at 130°. At this temperature it contains



The salt, like most of the rosaniline-salts, is very hygroscopic, a character which must not be lost sight of in the analysis of these compounds.

The monacid chloride dissolves more readily in moderately strong hydrochloric acid than in water. If this solution, gently warmed, be mixed with very concentrated hydrochloric acid, it solidifies, on cooling, into a network of beautiful brown-red needles, which have to be washed with concentrated hydrochloric acid and dried *in vacuo* over sulphuric acid and lime, since water decomposes them with reproduction of the monacid compound. The salt obtained by the action of concentrated hydrochloric acid is the compound with three equivalents of acid,

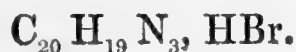


Exposed to 100° this salt gradually loses acid, the brown crystals becoming indigo-blue; and if the exposure be continued until the weight becomes constant, the original green salt with one equivalent of acid is reproduced, which was identified by analysis.

The two chlorides combine with *dichloride of platinum*. The compounds thus produced, being uncrystallizable, are not easily obtained in a state of purity. From platinum-determinations, which have given only approximative results, I infer that they contain respectively,



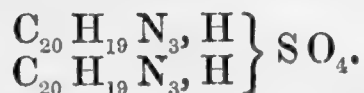
The *Hydrobromate of Rosaniline* resembles in every respect the hydrochlorate; it is even more difficultly soluble than the latter; it contains



Hydriodate of Rosaniline.—Green, very difficultly soluble needles of the composition

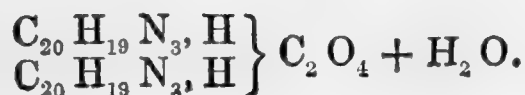


Sulphate of Rosaniline is readily obtained by dissolving the free base in boiling dilute sulphuric acid. On cooling, the salt is deposited in green metal-lustrous crystals, which by one recrystallization become perfectly pure. At 130°, at which temperature it loses a small quantity of water, the formula of the salt is



The acid sulphate crystallizes with difficulty. I have not analysed it.

Oxalate of Rosaniline.—The preparation and properties of this salt are similar to those of the sulphate. The salt retains at 100° one equivalent of water, and is at this temperature represented by the formula



The water may be expelled at a higher heat ; but the temperatures at which the water is lost and the salt commences to be decomposed are so close to each other, that it is not quite easy to obtain the salt in the anhydrous state. I have not been able to procure an oxalate with a larger amount of acid.

Acetate of Rosaniline.—This is probably the finest salt of the series. Mr. Nicholson has obtained it in crystals an inch in diameter, which, on analysis, were found to be the pure monacid acetate, viz.,



The acetate is one of the more soluble salts, both in water and in alcohol ; it cannot be conveniently recrystallized.

The *Formiate of Rosaniline* is similar to the acetate.

Of the remainder of the salts of this base I may mention the *Chromate*, which is obtained by adding chromate of potassium to the solution of the acetate in the form of a brick-red precipitate, becoming a green, crystalline, almost insoluble powder on ebullition with water.

The *Trinitrophenate* also deserves to be noticed ; it crystallizes in beautiful reddish needles, likewise very difficultly soluble in water, which contain



Multiplied and varied though the analytical results may be which support the formulæ of rosaniline and its compounds, it appeared desirable to seek additional experimental evidence for the expressions derived from simple analysis. With this view I have studied the products of decomposition of rosaniline. They are both numerous and interesting. I must limit myself to-day to quote one or two compounds which claim some attention, not only because they unmistakeably confirm the formula which I have proposed, but also on account of the light which they throw upon the nature of the class of substances to which rosaniline belongs.

Action of reducing agents upon Rosaniline.—This action appeared to promise the simplest mode of controlling the formula of the new base. My anticipations have not been disappointed. Rosaniline is readily attacked by nascent hydrogen or sulphuretted hydrogen. A solution of the base in hydrochloric acid, when left in contact with metallic zinc, is rapidly decolorized. The liquid thus produced contains, together with chloride of zinc, the hydrochlorate of a new triamine, which, perfectly colourless as it is in the free state and in its saline compounds, I propose to designate by the term *Leucaniline*. The separation of the new compound from the zinc being tedious and troublesome, I prefer to prepare it by the action of sulphide of ammonium. A salt of rosaniline, when digested for some time with sulphide of ammonium, furnishes a yellow, half-fused, scarcely crystalline, brittle compound, which constitutes the leucaniline in a state approaching purity. It is, however, by no means necessary to employ for the preparation of this compound a rosaniline-salt in the pure state. In most cases I have produced the leucaniline from the commercial article sold under the name of fuchsine or magenta.

To purify the product thus obtained, the yellow resinous mass is powdered, washed with water to remove the sulphide of ammonium,

and dissolved in dilute hydrochloric acid, when sulphur, together with impurities, are left behind. The dark-brown solution thus obtained yields, with concentrated hydrochloric acid, a copious crystalline precipitate, which, according to the degree of purity of the commercial colouring matter, is either brown or yellow. Washing with concentrated hydrochloric acid, in which the precipitate is insoluble, effects a considerable purification; but in most cases it is necessary to repeat the process of dissolving in dilute and reprecipitating by concentrated hydrochloric acid once or twice. If the solution, before the last addition of concentrated acid, be heated to ebullition, the liquid remains clear, and the new chloride crystallizes out only on cooling. The crystals are beautiful, white, rectangular plates, which are, however, always very small. Recrystallization from water, in which they are extremely soluble, renders them perfectly pure. Or they may be dissolved in alcohol and precipitated by ether, in which they are quite insoluble.

The chloride thus purified yields, on addition of ammonia, the leucaniline as a dazzling white powder, which assumes the faintest rosy tint when left for some time in contact with the atmosphere of the laboratory. It is scarcely soluble in cold, very little soluble in boiling water, from which it is deposited, on cooling, in the form of small crystals. It is very soluble in alcohol, and, although less so, in ether. I have not been able to get it in good crystals from these solvents. The best solvent appears to be a solution of the chloride above described, in which leucaniline is freely soluble, and from which, on cooling, it is deposited in the form of interlaced needles, which are frequently united to spherical aggregations. Leucaniline may be dried *in vacuo* over sulphuric acid without changing its colour. When slightly heated it becomes red, and at 100° it fuses to a deep-red liquid which, on cooling, solidifies to an indistinctly crystalline mass of lighter colour. Leucaniline is anhydrous. The analysis of the substance, dried *in vacuo*, and at 100° , has given results which correspond to the formula



This formula has been verified by the examination of the chloride already mentioned, of a splendidly crystallized platinum-salt, and, lastly, of the nitrate, which may be likewise obtained in good crystals.

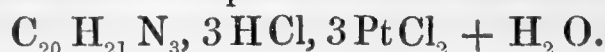
Hydrochlorate of Leucaniline.—The preparation of this compound has been mentioned. It is triacid, and retains, when dried *in vacuo*, one equivalent of water, its formula being



The salt cannot be dried at 100° , at all events in air; but the water may be expelled, although with great difficulty, by exposing the salt for a considerable length of time to 100° in a current of hydrogen. I have endeavoured to convert this compound into a salt with less acid by boiling the solution with an excess of leucaniline, but without result. The boiling solution deposits, on cooling, beautiful crystals of the base, the triacid salt remaining in solution.

Platinum-salt of Leucaniline.—On adding dichloride of platinum

to a moderately concentrated, gently warmed solution of the chloride, a splendid bright orange-yellow platinum-salt separates, on cooling, in well-formed prisms, generally aggregated to triangular stars. The salt is difficultly soluble in cold water; boiling water decomposes it. At 100° this salt retains one equivalent of water, which can be expelled, although with difficulty, at higher temperatures. Numerous analyses of this beautiful compound have established the formula



Nitrate of Leucaniline.—Well-formed white needles, soluble in water and alcohol, insoluble in ether. The salt is rather difficultly soluble in nitric acid. When dried *in vacuo* this salt contains



I have not succeeded in separating the water of crystallization, the salt being decomposed at 100° .

The salts of leucaniline are, in general, well crystallized. They are all very soluble in water, and precipitated from the aqueous solution by the addition of the respective acids. The sulphate crystallizes readily. I have submitted leucaniline to the action of disulphide of carbon, chloride of benzoyl, and several other agents. In all these cases new compounds are generated, some of them remarkable for their crystallizing power. The study of these substances does not belong to the present inquiry; I shall return to them in a future communication, in which I propose to examine the constitution of rosaniline and leucaniline, and their derivation from aniline. The object of the present Note was only to fix the composition of the two new bases, and their mutual relations to each other. This relation, as will be obvious by a glance at their formulæ, is of the simplest kind. In the anhydrous condition the two substances respectively contain,



Leucaniline differs from rosaniline simply by containing two equivalents of hydrogen more. The two bases hold to each other the relation which obtains between blue and white indigo :



Leucaniline, as might have been expected, is readily reconverted into the red colouring matter by oxidizing agents. The reaction succeeds with peroxide of barium, perchloride of iron, or chromate of potassium. On gently heating the colourless solution of the chloride with one of these reagents, the liquid at once reassumes the splendid colour of the rosaniline-salts. An excess of the oxidizing agents is, however, to be avoided, lest the action should go too far and the re-generated compound be converted into further products of oxidation. Both rosaniline and leucaniline, when submitted to protracted ebullition with highly oxygenated compounds, yield a brown amorphous powder, the composition of which remains at present unknown.

The two bases which I have described in the preceding paper are the prototypes of two series of homologous colouring matters which

cannot fail to be obtained from the homologues of aniline. Toluidine appears to yield perfectly similar bases. I have not, in the present Note, examined into the nature of the reaction by which aniline is transformed into rosaniline; in most of the processes which give rise to this substance, it is accompanied by several other bases, the study of which is not yet completed. Nor am I at present in a position to offer any definite opinion regarding the constitution of the new compounds, tempting though it appears to venture on speculations. It is in the hope of rendering the formulæ of the new bases more transparent that I have commenced to examine some of the products of decomposition. Their study is likewise far from being completed; but I may mention, even now, that both rosaniline and leucaniline when in nitric solution, are powerfully acted upon by nitrous acid, new bases being thus generated, the platinum-salts of which are remarkable for their fulminating properties. A splendid crystalline base also deserves to be mentioned, which, associated with aniline, appears among the products of distillation of rosaniline.

The results obtained in the further prosecution of these studies I propose to lay before the Royal Society in a future communication.

“On the Integration of Simultaneous Differential Equations.” By George Boole, Esq.

March 13.—“An Account of some Experiments with Eccentric Oblate Bodies and Disks as Projectiles.” By R. W. Woollcombe, Esq.

March 20.—Major-General Sabine, President, in the Chair.

The following communication was read:—

“Suggestions for the Attainment of a Systematic Representation of the Physical Aspect of the Moon.” By John Phillips, M.A., LL.D., F.R.S., Reader in Geology in the University of Oxford.

I. SKETCH OF THE PROGRESS OF SELENOGRAPHY.

(a) *By Eye-draughts and Micrometry.*

1. Beginning with the labours of Hevelius (1647), maps of the moon, embracing the whole, and signaling special parts, have been repeated by Riccioli (1651), Cassini (1680), Lalande (1787), T. Meyer (1748), Lambert-Schröter (1791), Lohrmann (1824), Beer and Mädler (1836).

2. The degree in which these laborious efforts may be regarded as meeting the wants of “Selenography,” is about equal to that in which the maps of England of the last century satisfy the requirements of physical geography; and in the same proportion as the great one-inch Ordnance Map of 1862 is superior to the old Chart of 1800, so should be the new drawings of the features of the moon to the older delineations.

3. That such drawings are attainable by the patient employment of modern instruments, in hands capable of good sketching, is, I believe, not doubted by any competent observer with either achromatic or reflecting telescopes having equatorial mounting. If any one

doubts, let him compare the Copernicus of Mädler with the Copernicus of Secchi ; nay, I may venture to ask that my own Gassendi be placed side by side with that of any of the charts already named.

4. The results likely to be attained by such a series of careful drawings of special parts of the moon's surface, in one branch of scientific research, are recognized by Mr. Conybeare in his Report on Geology to the British Association in 1832. Indeed, it may be boldly affirmed that a competent theory of volcanic action can hardly be regarded as having been adequately tested, much less completed, without a careful study of the magnificent volcanic surface of the moon, where for the most part the consolidated products of a long train of igneous eruptions are exhibited as clearly as in the celebrated region of Auvergne.

5. Considerations of this kind pressing upon Lord Rosse, Dr. Robinson, General Sabine, and other persons acquainted with the growing power of telescopes, and the necessity of organizing a system for the use of them on the moon, induced the British Association, assembled at Belfast in 1852, to constitute a Committee, consisting of the Earl of Rosse, the Rev. Dr. Robinson, and Professor Phillips, for the purpose of drawing up a Report on the physical character of the moon's surface as compared with that of the earth.

6. Acting as Secretary to this Committee, Professor Phillips forwarded invitations to fourteen selected observers, in Great Britain and Ireland, the United States, and several localities in Europe, known to be in possession of adequate instrumental power, or willing to provide it. To each observer a certain limited tract was offered, his peculiar work, but everyone was requested to add whatever information he might judge useful relating to other parts of the moon's surface.

7. The answers to these invitations were for the most part favourable as to good intentions ; but in several cases want of adequate leisure, sometimes want of health, sometimes other causes were mentioned ; and practically it was found that very few of the selected observers sent contributions which fulfilled the wishes of the Committee, even as preliminary surveys. The Secretary of the Committee, indeed, constructed an equatorial of large size for his own share of work, mounted it in the open air, made photographic and eye-drawings, and completed a sketch of his appointed region on the 19th of May, 1853, which sketch has been in the hands of the Royal Society. He thus established, to the satisfaction of several friends, the facility of carrying out the desires of the Committee, and would have taken up fresh districts, on every suitable occasion, but for the change of his residence from York to Oxford. The instrumental mounting being specially fitted for York and the circumstances of his residence there, he was unable to continue his work at Oxford ; and several years, as far as this problem is concerned, have been lost to him for want of an instrument of adequate power and suitable construction, conveniently placed and always at command.

8. Mr. Nasmyth, several years since, employed his fine reflector, with a peculiar apparatus for drawing, in these representations of the moon, which have justly earned for him a reputation in philosophic art of which even the inventor of the steam-hammer may be justly proud.

He has lately preferred to use for his eye-draughts a fine achromatic by Cooke, of York,—the same instrument which has been turned with such unexpected results to a scrutiny of the solar spots.

Professor Smyth of Edinburgh, and Professor Challis of Cambridge, made examinations and preliminary sketches of the Mare Crisium, Plato, and other interesting objects; the former artist employing oil-colours in his scene-painting.

(b) *By Photography.*

9. Meantime a new and beautiful art was making itself auxiliary to the delineation of the moon,—first by the silver plate of Daguerre, afterwards by the increasingly sensitive collodion surface. The great achromatic of Cambridge, U.S., under the hands of Bond and Whiffle, gave results of much promise; at first the light-pictures were of the full moon, 2 inches diameter on daguerreotype plates*; afterwards we saw larger representations of the crescent moon, with stronger lights and shadows on the ridges and in the hollows, several inches across (1851–53).

10. While observing with the great reflector at Birr, every one was struck with the probability that almost instantaneous pictures could be obtained of the moon, stars, and planets, by the amazing quantity of light brought to the focus of that magnificent instrument. Some trials had indeed been made in 1852 by the distinguished constructor and Mr. Woods; but I am not aware of the results of later experiments with the great reflector. In 1853 I gave much attention to the use of collodion, and employed an apparatus attached to my achromatic (of 11 feet focus and 6 inches diameter), by which at first pictures of 1·2 inch diameter, and finally others from 2 to 3 inches were obtained, in times gradually reduced from 5' to 30'' and less. I still possess many of these pictures; the best, however, was destroyed in attempting to print from it.

11. Somewhat earlier than these trials of mine were the first efforts of Mr. De la Rue, of which I was not aware. These efforts have from year to year been rewarded with still increasing success, till we have had from his skilful hands maps of the full moon of positive value, and stereographic pictures of admirable beauty. These researches are still in progress, with every prospect of reaching a point from which eye-draughts may be started on a fresh basis for a systematic scrutiny of all parts of the moon, and the construction of maps on the scale of $\frac{1}{20}$ th of an inch to a mile on the middle part of the moon's face (or as the moon would appear under a power of 1000).

12. Experiments rewarded by considerable success were completed by the Liverpool Photographic Society in 1854; and several of their valuable drawings of the moon, magnified to a large scale, were exhibited at the Meeting of the British Association in Liverpool, along with one of mine similarly handled.

II. PROPOSAL OF A METHOD FOR FURTHER PROGRESS.

13. By the labours, for the most part uncombined, of the last ten years, we have not achieved much beyond laying the foundation for

* Kosmos, iii. 2. 362.

further progress. We have acquired, by means of photography, a general view of the whole moon as to its proportions of light and shade, the degrees of light of different parts of its surface, the direction of the light-streaks, and other phenomena, better than eye-draughts and micrometry could furnish.

By eye-draughts and micrometry alone many of the “mountains” and “seas” of the moon have been sketched in beautiful landscapes by Nasmyth and Smyth; and two “ring mountains” have been surveyed and drawn in detail by Secchi (Copernicus) and Phillips (Gassendi).

The next ten years may, doubtless, be justly expected to give an equal rate of progress; photographic foundation will be made more effective for the whole moon and for different phases of the moon; and we may add, by individual and sporadic efforts, a few more ring mountains to our meagre catalogue of objects examined. It appears to me, however, that more than this can be attained, and ought to be attempted, on a plan of continuous work, by means of one instrument devoted to a survey of selected parts of the moon, and I proceed to explain my views.

14. By Mr. De la Rue's exertions principally, photographs of the moon have become an essential element in the desired delineations, and an impression is sometimes felt that by some possible further improvement in that wonderful art, eye-drawing may be dispensed with. This, I am persuaded, can never happen; but there is in my mind the firmest conviction that eye-drawing, founded on a basis of form obtained by photography, will produce results as to details of the moon's peculiarities which light-pictures alone can never reach. For whether the large photographs, on the scale of 100 inches to the moon's diameter, which we desire to obtain, are to be had by enlarging the primary pictures of 1 or 2 inches, or by direct photographs on a larger scale, it seems impossible to escape from some want of definition, by reason of the imperfect surfaces used, or by reason of the inexact following of the moon as she changes her rate or alters her declination. I know this latter error to be very likely of occurrence, even with disks taken beyond the negative eye-piece, with excellent clockwork movement, and am, on this account, the more ready to applaud Mr. De la Rue, whose skilful hands have so well mastered that and other difficulties. I cannot too strongly express my sense of the great value of the light-pictures obtained by that gentleman—as *a basis of form* on which to construct eye-draughts, showing the mind's interpretation of what the eye sees on the moon, but fails to discover in the finest photograph.

15. Reflecting on the comparatively very small degree of success which has rewarded the combination instituted nearly ten years since by the British Association,—remembering that instrumental means have been improved, while the scientific interest in a knowledge of the moon's peculiarities has not diminished,—it appears to me possible to obtain a larger measure of success by a vigorous effort in a different direction. It appears to me that, instead of requesting gentlemen who possess instruments already engaged in other researches to turn them to selenography and make drawings in which they may have no special interest, it will be better to carry a good

instrument to an observer interested in the survey of the moon, and willing for a limited time to use his exertions for the accomplishment of a definite object. In my own case I feel sure that this would succeed ; and I believe that my case is essentially that of many intelligent observers of the moon accustomed to extra-meridional observations.

16. The first desideratum then is an Equatorial Instrument, constructed with the conditions of ample optical power,—great steadiness,—delicate adjustment, including a sufficient range for latitude,—the usual circular and micrometrical readings,—clock-movement, &c.,—so that it may be in every point of view adapted for special observations of the moon (sun, planets, comets, &c. may also be observed), and be available for many years, in the same optical and space-measuring condition. According to my view, founded on experience with various instruments, it must be an achromatic, mounted on a transportable solid stand, placed under the roof of a removeable observatory, capable of holding a clock and, if need be, a small transit. The object-glass should be of 6 inches diameter, the focal length 16 or 17 diameters.

Such an instrument has actually been made by my direction ; it is finished, and stands complete in the workshop of the skilful artist, whose name is a guarantee of excellence, Mr. T. Cooke of York. Thus the first requisite to give effect to my proposal is practically reached.

17. The second desideratum is that the instrument shall become the property of some scientific body constituted for long endurance, and endowed with so much influence as to be able to give effect and gain adherence to a plan of continuous work, by definite persons, for such periods of time as each in succession may command. The instrument to be confided to each in succession, and mounted in a convenient manner for his use, at his home, during the time appointed. Each observer to furnish, at least once a year, an account of his observations, with drawings on the plan already detailed in the instructions furnished by the Moon Committee of the British Association. At the conclusion of his appointed period of observation, the instrument to be again at the disposal of the scientific body to which it belongs, either to be transferred to another observer, or to be again entrusted to the first observer, according as may seem best for the attainment of the object in view.

18. I entertain no doubt that, after the operation of one or two years, each yielding fruit, there will be no other difficulty of obtaining suitable observers than the difficulty of choice among several proper persons, who will be glad to give their services. To remove any difficulty as to the first trial, I presume to offer for the first two years my own services at Oxford ; having already sketched out a definite plan of work, which has not yet been attempted, and which I believe myself able to accomplish.

19. It would be no part of my plan to take photographs of the moon, but rather to obtain from other observatories the best examples of this kind of work, and devote every available hour to eye-sketching on a large scale of the exact appearance of selected parts of the lunar disk.

The drawings thus made, scrutinized and corrected in succeeding

years, would gradually and not very slowly grow up to complete eye-draughts of the moon, under the conditions of sunrise, mid-day, and sunset; and would themselves be again a starting-point for the guidance of even closer scrutiny, with the greatest telescopes and the sharpest eyes.

20. Finally, my proposal, if allowed to make one, would be, that, for the purpose of securing a series of satisfactory drawings of the physical features of the moon, a six-inch achromatic, by Cooke, constructed for the purpose, be purchased out of the funds of the Government Grant Committee, and held by a Board composed of three Members of the Royal Society, to be nominated in the first instance and the number afterwards filled up by the Council of the Royal Society, in trust for the use of observers to be appointed by the Board, each for a limited period, and for a defined area of work: the drawings and observations to be communicated, at least once a year, to the Board. Cost of the instrument not to exceed 320 guineas, of a moveable house not to exceed £50.

LXXV. *Intelligence and Miscellaneous Articles.*

A REMARKABLY EASY MODE OF APPROXIMATING TO THE CIRCUMFERENCE OF THE CIRCLE. BY S. M. DRACH, F.R.A.S., F.R.G.S.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,

WRITE down

$$\begin{array}{r}
 3\cdot00000,0 \\
 - \quad 800,0 \\
 - \quad \quad 7 \\
 \hline
 A = 2\cdot99199,3 \\
 + \frac{A}{20} = +14959,965 \\
 \hline
 B = 3\cdot14159,265 \\
 + \frac{A}{10^9} = \quad \quad \quad 29,91993 \\
 + \frac{2A}{10^{10}} = \quad \quad \quad 5,98398,6 \\
 \hline
 C = 3\cdot14159,26535,90391,6 \\
 \text{Sub. } \frac{2A}{10^{13}} = \quad \quad \quad -598,39860 \\
 \hline
 D = 3\cdot14159,26535,89793,20140 \\
 \text{Add } \frac{A}{8\cdot10^{16}} = \quad \quad \quad 3740 \\
 \hline
 E = 3\cdot14159,26535,89793,23880 \\
 \text{True value } 3\cdot14159,26535,89793,23846 \\
 \text{Error } + \cdot00000,00000,00000,00034
 \end{array}$$

B is true to the 8th dec., C. to the 10th, D to the 16th, E to the 18th.

The converse:—

Write $\cdot 050525 \times 9 = \cdot 45472,5 = a$

$$\frac{7}{10} a = \cdot 31830,75$$

$$+ \frac{a}{2 \cdot 10^5} = 22736,25$$

$$\hline 31830,97736,25$$

$$+ \frac{1}{20} \cdot \frac{a}{2 \cdot 10^5} = 1136,8125$$

$$+ d^{\circ} \times \frac{1}{10^4} = 11368,125$$

$$- d^{\circ} \times \frac{1}{10^2} = -11,36812,500$$

$$\text{Sum} = \cdot 31830,98861,80805,625$$

$$\frac{\text{Sum}}{10^{11}} = 3183,099$$

$$- \frac{1}{16} d^{\circ} = -198,944$$

$$\hline \cdot 31830,98861,83789,780$$

$$\text{Then} \quad \cdot 31830,98861,83790,671$$

$$\text{Error} \quad - \cdot 00000,00000,00000,891$$

S. M. DRACH.

39 Howland Street, London,
October 15, 1862.

ON THE SOLAR ENERGY AS MANIFESTED IN THE AUTUMN OF 1861
AND A PORTION OF THE SPRING OF 1862. BY W. R. BIRT, ESQ.

During the summer of 1860, both previous to and in connexion with the great outburst of solar energy that occurred towards the end of June in that year, I noticed a very marked monthly maximum and minimum of solar spots; and while there existed indications of an intermittent action of the solar force, I was satisfied that the maxima and minima I had frequently noticed were dependent on the rotation of the Sun.

Regarding each group of spots, single spots, if unattended by smaller ones, or separate nuclei if they were sufficiently distinct to warrant the appellation, as centres of energy, I laid down from time to time curves of which the centres of energy were the ordinates.

Taking the solar meridian turned directly towards the Earth at noon of the 9th of July 1861 equal to 270° , and employing the period of $27^d 7^h 10^m$ as that of the synodical rotation of the Sun, I set off 27 abscissæ, each being distinguished by a Roman numeral i.

Phil. Mag. S. 4. No. 164. Suppl. Vol. 24.

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to xxvii. preceded by the zero ; and as the rotation of the Sun apparently carries a meridian about $13^{\circ}19'$ westwardly per diem, each ordinate may represent any solar meridian within the limits of $13^{\circ}19'$: thus the zero may represent any meridian between $353^{\circ}41'$ and $6^{\circ}59'$, and xiv. any one between $178^{\circ}02'$ and $191^{\circ}20'$, and so on.

I have the pleasure of submitting to the Society the curves of five synodical rotations from observations in August, September, and October 1861, and April and May 1862*. Each presents a well-marked maximum, and the indications of minima are also apparent. The maxima are, without exception, acuminated, showing in each case not so much a diminution of energy as a removal from observation of the active solar surface by rotation.

The most instructive feature of the curves consists of the portions of the surface of the solar globe indicated as manifesting the greatest energy. 1861, August, the meridians 23° to 208° manifested the greatest energy during the period ; indeed, a much greater activity than the opposite hemisphere. During the next rotation the energy was greatly subdued ; the curve, nevertheless, exhibited *an increase* of force between 125° and 297° ; and by projecting the September under the August curve, the similarity of features, as regards the increase and diminution of activity, is unmistakeable.

The October curve, after presenting us with a solar hemisphere perfectly quiescent, meridian 42° , gives us increased action from 108° to 293° ; indeed there had been an increase of activity since 42° was central. Collecting these areas of increased energy, we have a portion of the solar surface extending over 274° , manifesting considerably more energy than the remaining portion of 86° . The following are the meridians of maxima :—August, 156° ; September, 138° ; October, 200° ; and this extent of 62° , or thereabouts, may be regarded as the locality of greatest energy for those months, as referred to heliographic longitudes. The heliographic latitudes of solar energy are well known.

1862, the April curve exhibits subdued energy from meridians 14° to 173° , then increased energy to 332° , with a maximum at 292° . The May curve manifests during its earlier portion more activity than the April, with a minimum at 156° . This was the meridian of maximum in August ; so that the activity on the hemisphere, of which 156° is the central meridian, is now greatly reduced ; in fact the direction of the curve is reversed. From this meridian the curve manifests increased energy. The two curves for April and May present as perfect an agreement in their features as those of August, September, and October 1861. At the present time the hemisphere of greatest energy is marked by the central meridian 300° , or thereabouts.—*Monthly Notices of the Roy. Astronom. Soc.* Nov. 14, 1862.

* The drawings of these curves were exhibited at the Meeting in June last.—ED.

INDEX to VOL. XXIV.

- ACETONES**, on the conversion of, into alcohols, 309.
- Acetylene, researches on, 22, 187; on the occurrence of a compound of, in copper gas-pipes, 384.
- Air, on the temperature of the inferior layers of the, 244.
- Air thermometer, on some properties of the, 263.
- Aldehyde, on the conversion of, into alcohol, 191.
- Alloys of lead-zinc and bismuth-zinc, on the, 317; of gold-tin, 320.
- Alps, on the conformation of the, 169, 377.
- Aluminate of baryta, on soluble, 27.
- Aluminium bronze as a material for the construction of philosophical instruments, on, 508.
- Ampèrian repulsion, observations on, 126.
- Amylene, on a new method of forming, 24; on the hydrate of, 386.
- Analysis, correlations of, 531.
- Ångström (A. J.) on the Fraunhofer-lines visible in the solar spectrum, 1.
- Aniline, on the colouring matters produced from, 547.
- Asterism, and Brewster's luminous figures, observations on, 497.
- Atkinson's (Dr. E.) chemical notices, 22, 185, 305, 381, 519.
- Atmosphere, on the aqueous vapour of the, 350.
- Auroræ boreales, on the, 51.
- Balloon ascents, on the determination of temperature and pressure in, 213.
- Barometer, on the lunar semidiurnal variation of the, 66.
- Barth (M.) on a new acid from milk-sugar, 529.
- Baryta, on the detection of, 520.
- Bauer (M.) on some new ethers, 305.
- Baxter (H. F.) on nerve force, 11.
- Bechamp (M.) on nitric compounds of starch, 526.
- Becquerel (M.) on the temperature of the inferior layers of the air, 244; on electro-chemical reductions, 383.
- Beetz (Prof. W.) on the colour of water, 218.
- Beilstein (M.) on the preparation of zinc-ethyle, 306; on hydracrylic acid, 308.
- Benoit (M.) on a mode of applying the electric light for mining purposes, 408.
- Benzoic acid, on new derivatives of, 190.
- Benzyle, on a new method of preparing, 24.
- Berthelot (M.) on acetylene, 22, 187.
- Bianchi (M.) on the combustion of gunpowder *in vacuo* and in various gaseous media, 407.
- Birt (W. R.) on the solar energy as manifested in 1861-1862, 561.
- Bismuth, on the oxides of, 28.
- Bolton (J.) on a clay deposit near Ulverston, 74.
- Bontemps (G.) on the window-glass of Pompeii, 78.
- Book, new:—Kirchhoff's Researches on the Solar Spectrum, 52.
- Boole (Prof. G.) on a question in the theory of probabilities, 80.
- Bose (M. von) on the lead-zinc and bismuth-zinc alloys, 317; on some gold-tin alloys, 320; on the influence of temperature on the electric conducting power of the metals, 405.
- Bouis (M.) on œnanthole, 385.
- Brewster (Sir D.) on the action of various coloured bodies on the spectrum, 441.
- Briegleb (M.) on nitride of magnesium, 381.

- Brodie (Prof.) on oxidation and dis-oxidation by peroxide of hydrogen, 393.
- Broun (J. A.) on the lunar semidiurnal variation of the barometer, 66 ; on the variation of magnetic declination, 67.
- Bunsen (Prof.) on the preparation of the rubidium compounds, 46.
- Caffeidine, researches on, 528.
- Cahours (M.) on American petroleum, 384.
- Calcium, on a carbide of, 526.
- Calculus of variations, on the principle of discontinuity in solutions of problems in the, 196.
- Campbell (D.) on the action of nitric acid upon pyrophosphate of magnesia, 380.
- Camphor, on the motion of, towards the light, 358 ; on the motions of, on the surface of water, 490.
- Camphrene, researches on, 530.
- Cannizzaro (M.) on some organic radicals, 24.
- Caproylene, on hydride of, 384.
- Carbon, on the determination of, in iron, 522, 525.
- Carlet (M.) on *œnanthole*, 385.
- Cayley (A.) on a transcendent equation, 19 ; on equations of the fifth order, 291 ; on the skew surface of the third order, 514.
- Challis (Prof.) on a mathematical theory of the vibrations of an elastic fluid, 135, 291 ; on the principle of discontinuity in solutions of problems in the calculus of variations, 196 ; on the explanation of phenomena of light on the hypothesis of undulations, 462.
- Chandler (C. F.) on a new metal in the native platinum of Oregon, 168.
- Chemical Notices from foreign journals, 22, 185, 305, 381, 519.
- Chlorides, on the violet flame of many, 417.
- Chromium, on the reduction of, from solution of the chloride, 328.
- Circle, on an easy mode of approximating to the circumference of the, 560.
- Clausius (Prof. R.) on the application of the theorem of the equivalence of transformations to the internal work of a mass of matter, 81, 201.
- Cocaine, researches on, 25.
- Cockle (J.) on a new species of differential equations, 37 ; on symbolical decomposition, 288 ; on the correlations of analysis, 531.
- Combustion, on the influence of atmospheric pressure upon some of the phenomena of, 232.
- Condensers, on the limit of the charge of, 495.
- Coniine, researches on, 388.
- Copper, on some basic salts of, 123.
- Crafts (J. M.) on the sulphide of ethylene, 387.
- Croll (J.) on Ampèrian repulsion, 326.
- Crova (M.) on the formation of cupreous acetylene in copper gas-pipes, 384.
- Crystallization, researches on, 395.
- Cuminyle, on a new method of preparing, 24.
- Davy (M.) on the electromotive force of voltaic piles, 76.
- Dawson (J. W.) on the land flora of the Devonian period in North-eastern America, 75.
- Debray (M.) on the volatilization of tungstic acid, 309.
- Deville (M.) on the production of Levyne, 26.
- Dioxyethylene, researches on, 23.
- Drach (S. M.) on an easy mode of approximating to the circumference of the circle, 560.
- Dufour (L.) on the density of ice, 167.
- Dumas (M.) on a mode of applying the electric light for mining purposes, 408.
- Duprey (M.) on the preparation of oxygenated water, 526.
- Earth, on the fluid theory of the figure of the, 409, 507.
- Ecgonine, on the production of the new base, 25.
- Eclipse, on the nature of the red protuberances on the sun's disk during a total, 302.
- Edlund (M.) on the thermal phenomena which accompany the changes in volume of solid bodies, 329.
- Electric force, on the relation of nerve force to, 11.
- light, on a mode of applying the, for mining purposes, 408.
- resistance, on the measurement of, 149.

- Electrolysis, on the production of vibrations and sounds by, 401.
- Electromotive force of voltaic piles, on the, 76.
- Electro-physiological researches, 311.
- Engelhardt (M.) on the detection of baryta and strontia, 520.
- Equation, transcendent, on a, 19; on a symbolic and biquadratic, 127; in differences, on the integral of the general, 436.
- Equations, differential, on a new species of, 37; of the fifth order, on, 193, 290.
- Equilibrium, on figures of, of a liquid mass without weight, 128.
- Ethers, on some new, 305.
- Ethylene, on the sulphide and sulpho-carbonate of, 387.
- Eye, on the diathermancy of the media of the, 176.
- Falconer (Dr. H.) on *Plagiaulax*, 240.
- Faraday (Prof.) on gas-furnaces, 162.
- Field (F.) on some basic salts of copper, 123.
- Films, on the generation of liquid, 128.
- Finck (M.) on some products of the decomposition of pyroracemic acid, 191.
- Fizeau (H.) on the modifications which the velocity of light experiences under the influence of heat, 492.
- Flames of the chlorides, on the prismatic spectra of the, 417.
- Fluid, elastic, mathematical theory of the vibrations of an, 135, 291.
- Forces of inorganic nature, on the, 57, 371.
- Forsberg (M.) on artificial tungstates, 28.
- Frankland (Dr. E.) on the influence of atmospheric pressure on some of the phenomena of combustion, 232.
- Franz (R.) on the diathermancy of the media of the eye, 176.
- Fraunhofer-lines visible in the solar spectrum, on the, 1.
- Friedel (M.) on the conversion of acetones into alcohols, 309.
- Gas-furnaces, observations on, 162.
- Gassiot (J. P.) on the heat developed at the poles of a voltaic battery during the passage of luminous discharges, 225.
- Gaudin (M.) on soluble aluminate of baryta, 27.
- Gaugain (M.) on the limit of the charge of condensers, 495.
- Gelis (M.) on hyposulphuric acid, 189.
- Geological Society, proceedings of the, 73, 165, 240, 323, 491.
- Geuther (M.) on artificial tungstates, 28; on nitride of magnesium, 381.
- Giles (W. B.) on the reduction of manganese from the protochloride by sodium-amalgam, 328.
- Gladstone (Dr. J. H.) on the atmospheric lines of the solar spectrum, 70; on the violet flame of many chlorides, 417.
- Gore (G.) on the production of vibrations and sounds by electrolysis, 401; on the nature and qualities of voltaic currents, 403.
- Graham (T.) on liquid transpiration in relation to chemical composition, 238.
- Greg (R. P.) on some meteorites, 534.
- Ground-ice, observations on, 241.
- Gunpowder, on the combustion of, *in vacuo*, and in various gaseous media, 407.
- Hamilton (Sir W. R.) on a symbolic and biquadratic equation, 127.
- Harbordt (M.) on the essential oil of rue, 529.
- Harkness (Prof. R.) on the position of the *Pteraspis*-beds in South Perthshire, 73; on the metamorphic rocks of the Banffshire coast, 165.
- Heat, on the dynamical theory of, 121, 173, 327; on the phenomena of, which accompany the changes in volume of solid bodies, 329; on the absorption and radiation of, by gaseous matter, 270, 337, 422.
- Hippuric acid, on new derivatives of, 190.
- Hlasiwetz (Prof.) on a new acid from milk-sugar, 529.
- Hofmann (Prof.) on the colouring matters from aniline, 547.
- Honeyman (Rev. D.) on the gold-fields of Nova Scotia, 165.
- Husemann (M.) on the sulphocarbonate of ethylene, 387.
- Huxley (Prof.) on some new Labyrinthodonts, 75.
- Hydracrylic acid, 308.

- Hyposulphuric acid, on a new method of preparing, 189.
 Ice, on the density of, 167.
 Induced current, on some properties of the, 249.
 Iodic acid, on the volumetric determination of, 188.
 Iodine vapour, on the abnormal dispersion of, 245.
 Iron, on the estimation of carbon and nitrogen in, 522.
 Jerrard (G. B.) on quintics, 193, 457.
 Joule (J. P.) on the history of the dynamical theory of heat, 121.
 Jukes (Prof. J. B.) on the mode of formation of some of the river-valleys in the south of Ireland, 323.
 Kobell (Prof. von) on asterism and Brewster's luminous figures, 497.
 Kuhlmann (M.) on salts of thallium, 525.
 Lamont (Prof.) on Dalton's theory of vapour, 350.
 Lamy (M.) on the new metal thallium, 185.
 Laplace's functions, on a geometrical proof of the fundamental principle of, 504.
 Leroux (F.-P.) on the refractive indices of bodies which only assume the gaseous condition at high temperatures, 245.
 Leucaniline, 553.
 Levyne, on the artificial production of, 26.
 Lieben (M.) on some new ethers, 305.
 Liebig (J.) on the preparation of the iodide of lithium, 29.
 Light, on the explanation of phenomena of, on the hypothesis of undulations, 462; reflected from a pile of plates, on the intensity of the, 480; on the modifications which the velocity of, experiences under the influence of heat, 492.
 —, electric, on a mode of applying the, for mining purposes, 408.
 Linnemann, on the conversion of sugar into mannite, 307.
 Lithium, on the atomic weight of, 26; on the preparation of the iodide of, 29.
 Logeman (Prof. W. M.) on Ampèrian repulsion, 126.
 Lössen (M.) on cocaine, 25.
 Magnesia, on the action of nitric acid upon the pyrophosphate of, 380.
 Magnetic declination, on the diurnal variation of, 67.
 — disturbance of 1859, on the, 315.
 — observations, on some, 542.
 Magnetism, terrestrial, on the cosmical features of, 97.
 Magnetizing steel needles by the current of a Leyden jar, on, 494.
 Mallet (Dr. J. W.) on the transit-velocity of earthquake-waves, 229.
 Manganese, on the reduction of, 328.
 Mannite, on the conversion of sugar into, 307.
 Matter, on the internal work of a mass of, 81, 201.
 Matteucci (Prof. C.) on certain electrophysiological phenomena, 311.
 Matthiessen (Dr. A.) on the influence of traces of foreign metals on the electric conducting power of mercury, 30; on the lead-zinc and bismuth-zinc alloys, 317; on some gold-tin alloys, 320; on the influence of temperature on the electric conducting power of the metals, 405.
 Mayer (J. R.) on the forces of inorganic nature, 371.
 Mendius (M.) on the nitriles, 189.
 Metal, on a new, 168.
 Metals, on the influence of temperature on the electric conducting power of the, 405.
 Meteorites, observations on some, 534.
 Milk-sugar, on a new acid from, 529.
 Miller (Prof.) on the crystalline form of a gold-tin alloy, 321.
 Mitscherlich (M.) on spectrum analysis, 519.
 Moon, on the changes in the apparent size of the, 326, 390.
 Nebulæ, observations on the, 237.
 Nerve force, observations on, 11.
 Nerves, on the secondary electromotor power of, 311.
 Nitriles, on some transformations of the, 189.
 Nitrite of ammonia, on the formation of, from atmospheric air, 520.
 Nitrogen, on the determination of, in iron, 523.
 Œnanthole, on some metamorphoses of, 385.

- Otto (M.) on some derivatives of benzoic and hippuric acids, 190.
- Oxycinchonine, researches on, 527.
- Paalzow (Dr.) on magnetizing steel needles by the current of a Leyden jar, 494.
- Parallel surfaces, on some applications of a theorem relating to, 39.
- Pelouze (M.) on American petroleum, 384.
- Phillips (Dr.) on the progress of selenography, 555.
- Plateau (Prof. J.) on figures of equilibrium of a liquid mass without weight, 128.
- Potter (Prof.) on the auroræ boreales, 51; on some properties of the air thermometer, 263; on the definition of the temperature of bodies, and on its measurement by thermometers, 447.
- Pratt (Archdeacon J. H.) on the fluid theory of the figure of the earth, 409, 507; on a geometrical proof of the fundamental principle of Laplace's functions, 504.
- Probabilities, on a question in the theory of, 80.
- Pyrroacemic acid, on some products of the decomposition of, 191.
- Quintics, on, 193, 457.
- Radiation, on internal, 474.
- Ramsay (Prof.) on the excavation of the valleys of the Alps, 377.
- Rankine (W. J. M.) on the exact form and motion of waves at and near the surface of deep water, 420.
- Regnault (V.) on the determination of temperature and pressure in balloon ascents, 213.
- Rieth (M.) on the preparation of zinc-ethyle, 306.
- Rijke (Prof.) on some properties of the induced current, 249.
- Roberts (Rev. W.) on some applications of a theorem relating to parallel surfaces, 39.
- Rodwell (G. F.) on the dynamic theory of heat, 327.
- Rosaniline, 547.
- Rosse (Earl of) on the nebulæ, 237.
- Rossi (M.) on some organic radicals, 24.
- Royal Institution, proceedings of the, 57, 162.
- Royal Society, proceedings of the, 66, 149, 225, 311, 392, 474, 542.
- Rubidium, on the preparation of compounds of, 46; on the occurrence of, 382.
- Rue, on the constitution of the essential oil of, 529.
- Sabine (General) on the cosmical features of terrestrial magnetism, 97; on some conclusions derived from the photographic records of the Kew declinometer, 542.
- Salter (J. W.) on fossil crustaceans, 166.
- Sandberger (Prof. D. F.) on some upper eocene fossils from the Isle of Wight, 76.
- Sarcosine, on the synthesis of, 529.
- Schœnbein (Prof.) on the formation of nitrite of ammonia from atmospheric air, 520.
- Schrader (M.) on the oxides of bismuth, 28.
- Schwanert (M.) on camphrene, 530.
- Selenography, sketch of the progress of, 555.
- Siemens's gas-furnaces, observations on, 162.
- Simmler (M.) on a fluorescent solution from cane-sugar, 192.
- Skew surface of the third order, on the, 514.
- Soda, on the preparation of pure caustic, 27.
- Spectrum, on the Fraunhofer-lines visible in the solar, 1; on the atmospheric lines of the solar, 70; observations on the solar, 407; on the action of various coloured bodies on the, 441.
- Spectrum analysis, observations on, 519.
- Starch, on nitric compounds of, 526.
- Stewart (B.) on the nature of the red protuberances seen on the sun's limb during a total eclipse, 302; on the great magnetic disturbance of 1859, 315.
- Stokes (G. G.) on internal radiation, 474; on the intensity of the light reflected from a pile of plates, 480.
- Strange (Lieut.-Colonel A.) on aluminium bronze as a material for the construction of philosophical instruments, 508.
- Strecker (Dr.) on oxycinchonine and caffeidine, 527.

- Strontia, on the detection of, 520.
 Sugar, on the conversion of, into mannite, 307.
 Sylvester (Prof. J.) on the integral of the general equation in differences, 436.
 Symbolical decomposition, on, 288.
 Temperature of bodies, on the measurement of, by thermometers, 447.
 Thallium, on the preparation and properties of, 185; on some organic salts of, 525.
 Thermometers, on the measurement of the temperature of bodies by, 447.
 Thomson (Prof. J.) on ground-ice in rivers, 241; on the calm lines often seen on a rippled sea, 247; on crystallization and liquefaction, 395.
 Thomson (Prof. W.) on the measurement of electric resistance, 149.
 Tomlinson (C.) on the motion of camphor towards the light, 358; on the motions of camphor on the surface of water, 490.
 Transformations, on the principle of the equivalence of, 81, 201.
 Transpiration, on liquid, 238.
 Troost (M.) on the equivalent of lithium, 26.
 Tungstates, on artificial, 28.
 Tungstic acid, on the volatilization of, 309.
 Tyndall (Prof.) on force, 57; on the conformation of the Alps, 169; on Mayer, and the mechanical theory of heat, 173; on the absorption and radiation of heat by gaseous matter, 270, 337, 422.
 Ullgren (M.) on the determination of carbon and nitrogen in iron, 522.
 Undulatory theory, observations on the, 462.
 Uvitinic acid, 192.
 Van Breda (Prof.) on Ampèrian repulsion, 126.
 Vapour, on Dalton's theory of, 30.
 Vibrations, on the hydrodynamical theory of, 135, 291.
 Vincent (C. W.) on the reduction of chromium from solution of the chloride, 328.
 Vogt (Dr. C.) on the influence of traces of foreign metals on the electric conducting power of mercury, 30.
 Volhard (M.) on the synthesis of sarcosine, 529.
 Voltaic battery, on the heat developed at the poles of a, 225.
 — currents, on the nature and qualities of, 403.
 — piles, on the electromotive force of, 76.
 Water, on the colour of, 218.
 Waves, on the form and motion of, 420; on the transit-velocity of earthquake-, 229.
 Weber (Dr.) on the crystals of the sulphuric-acid chambers, 310.
 Weiss (Dr. A.) on the solar spectrum, 407.
 Weltzien (M.) on the formation of iodic acid, 188.
 Wertheim (M.) on coniine and derivatives, 388.
 Whitaker (W.) on the western extremity of the London basin, 74.
 Wich (M.) on the determination of carbon in iron, 525.
 Window-glass of Pompeii, examination of the, 78.
 Wöhler (Prof.) on cocaine, 25; on the preparation of pure caustic soda, 27; on carbide of calcium, 526.
 Wurtz (M.) on dioxyethylene, 23; on a new method of forming amylene, 24; on aldehyde, 191; on hydrate of amylene, 386.
 Zeno (T.) on changes in the apparent size of the moon, 390.
 Zinc-ethyle, on the preparation of, 306.

END OF THE TWENTY-FOURTH VOLUME.

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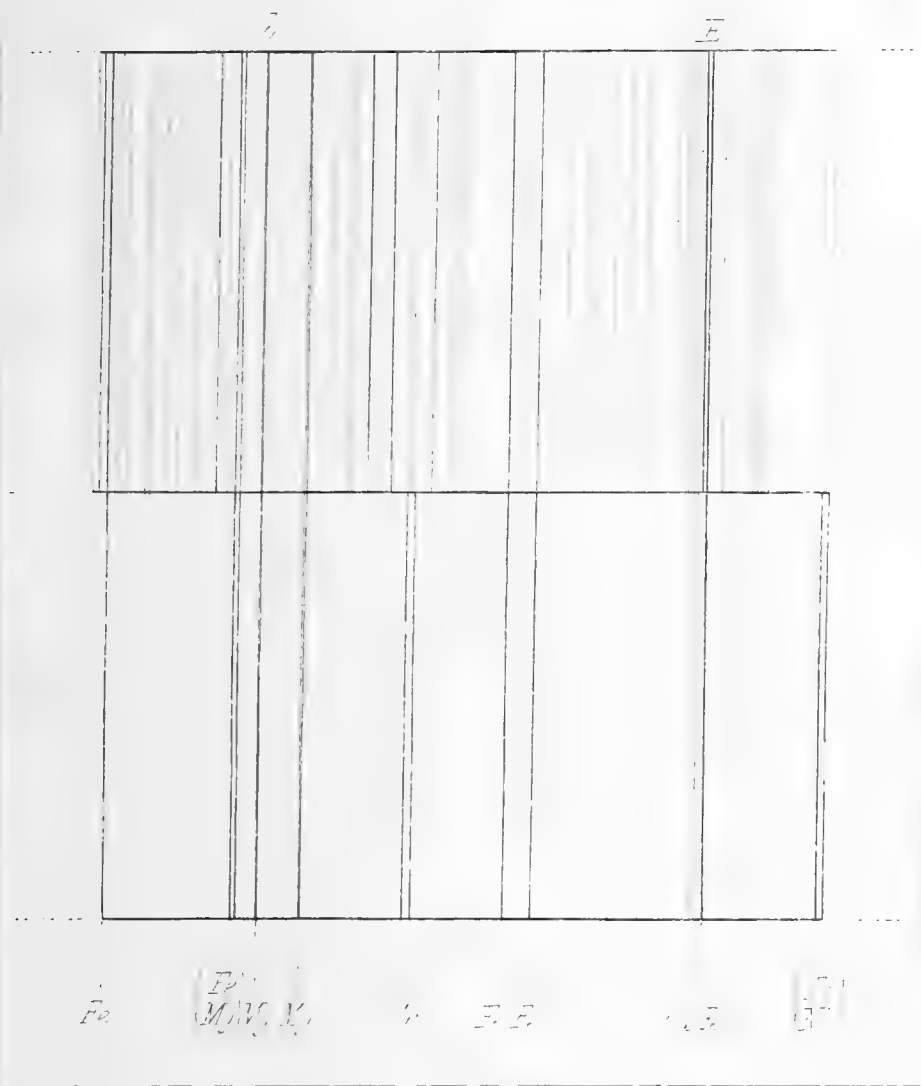


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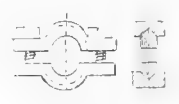
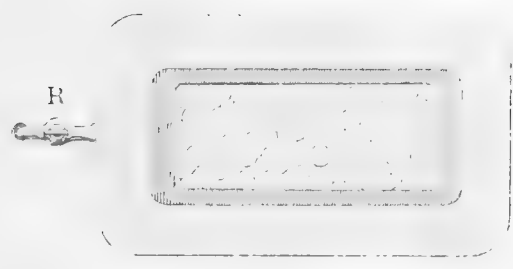


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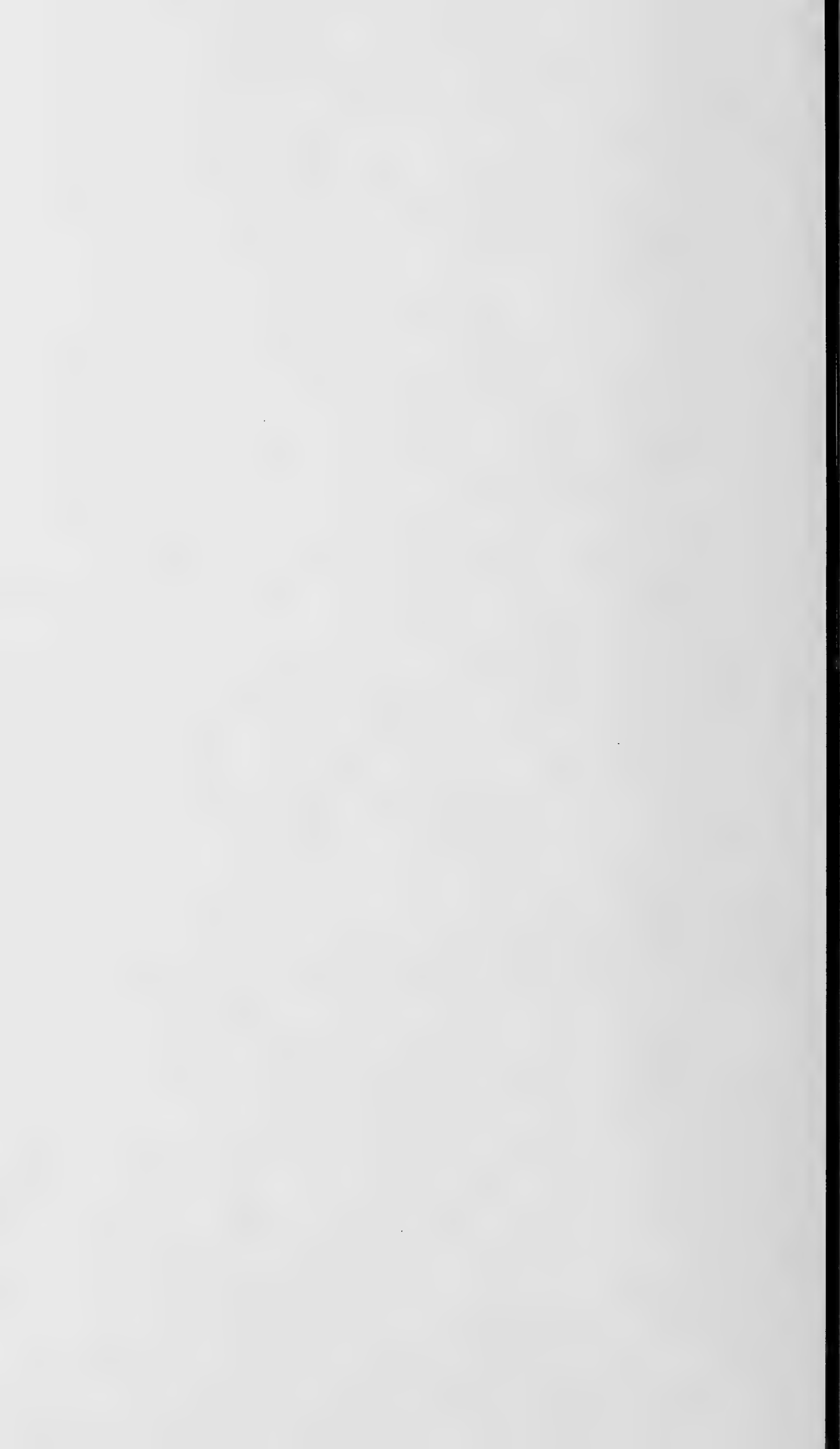


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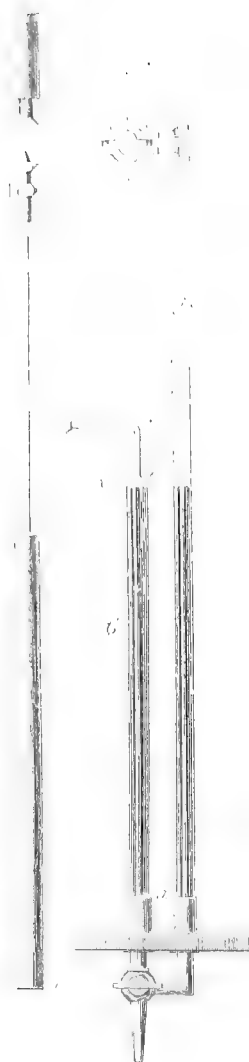
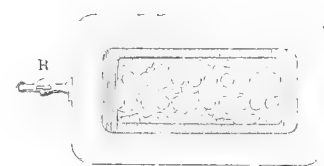
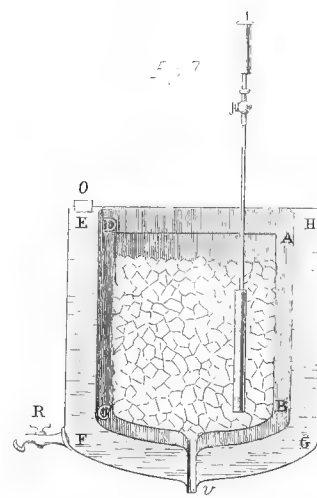
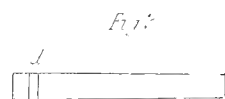
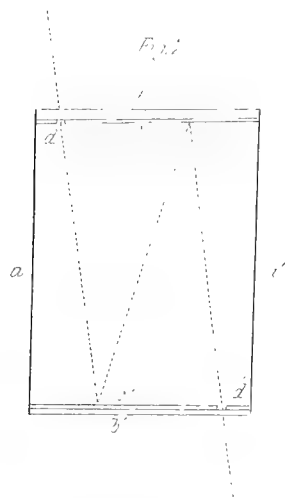
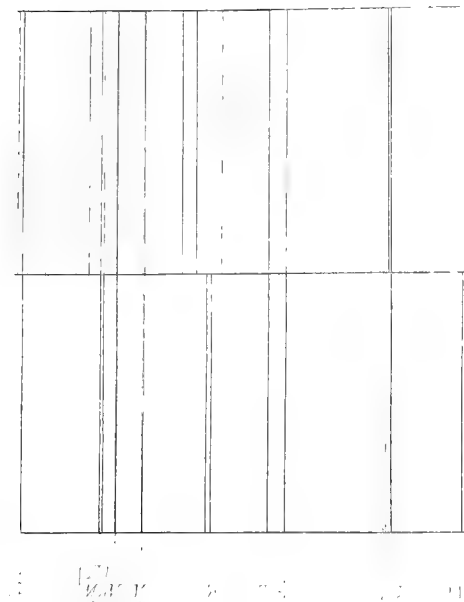
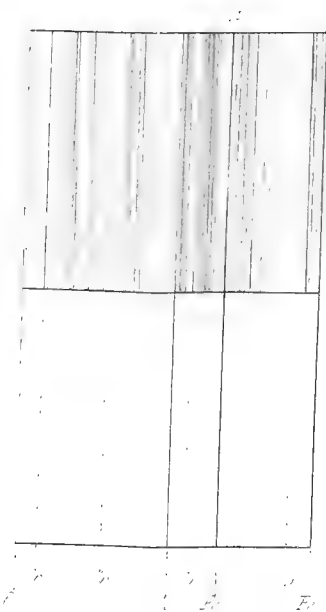
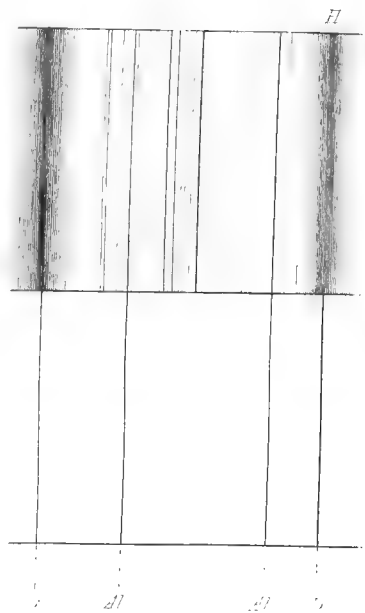


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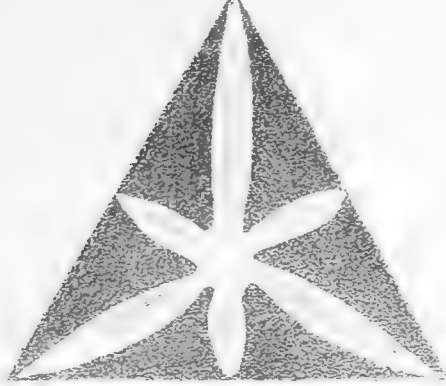


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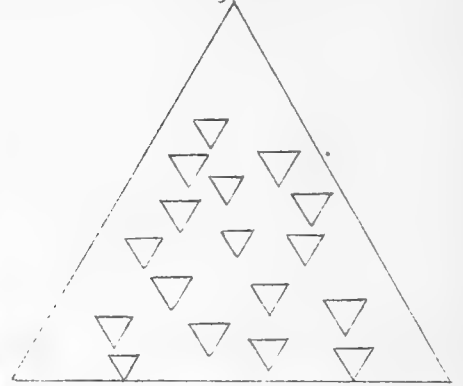


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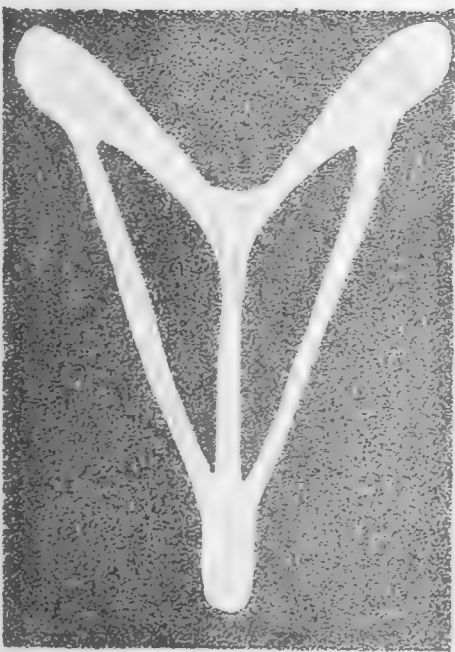


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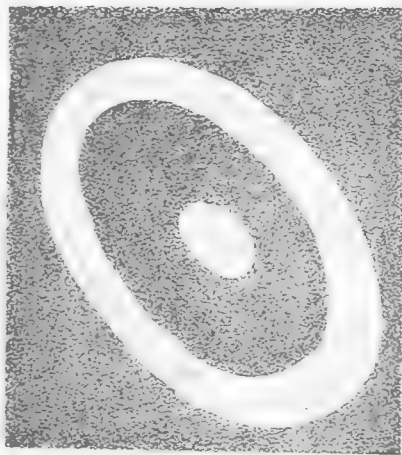


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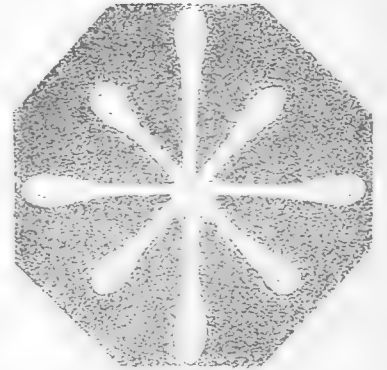


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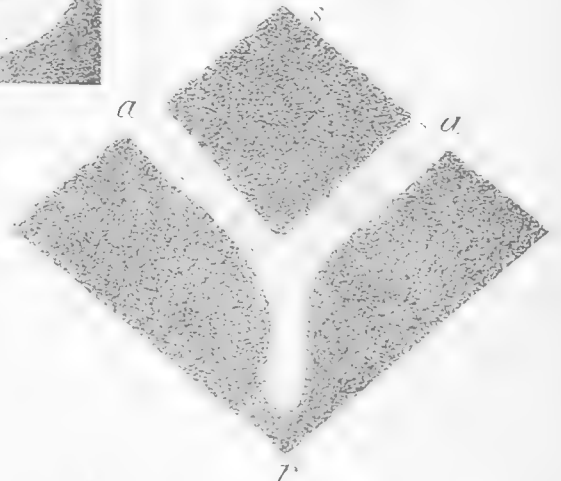
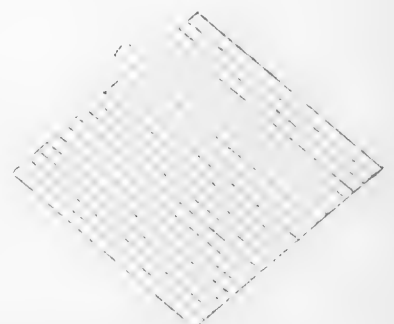
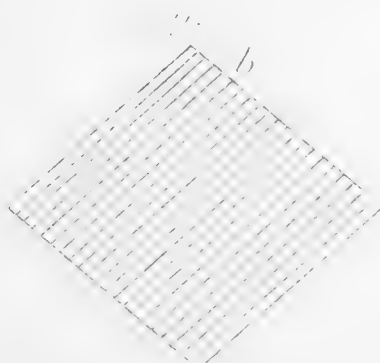
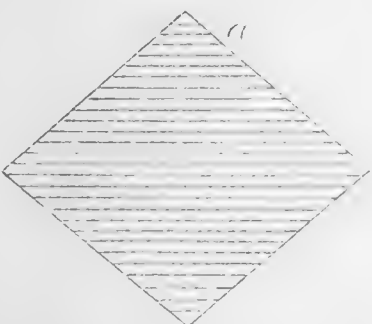


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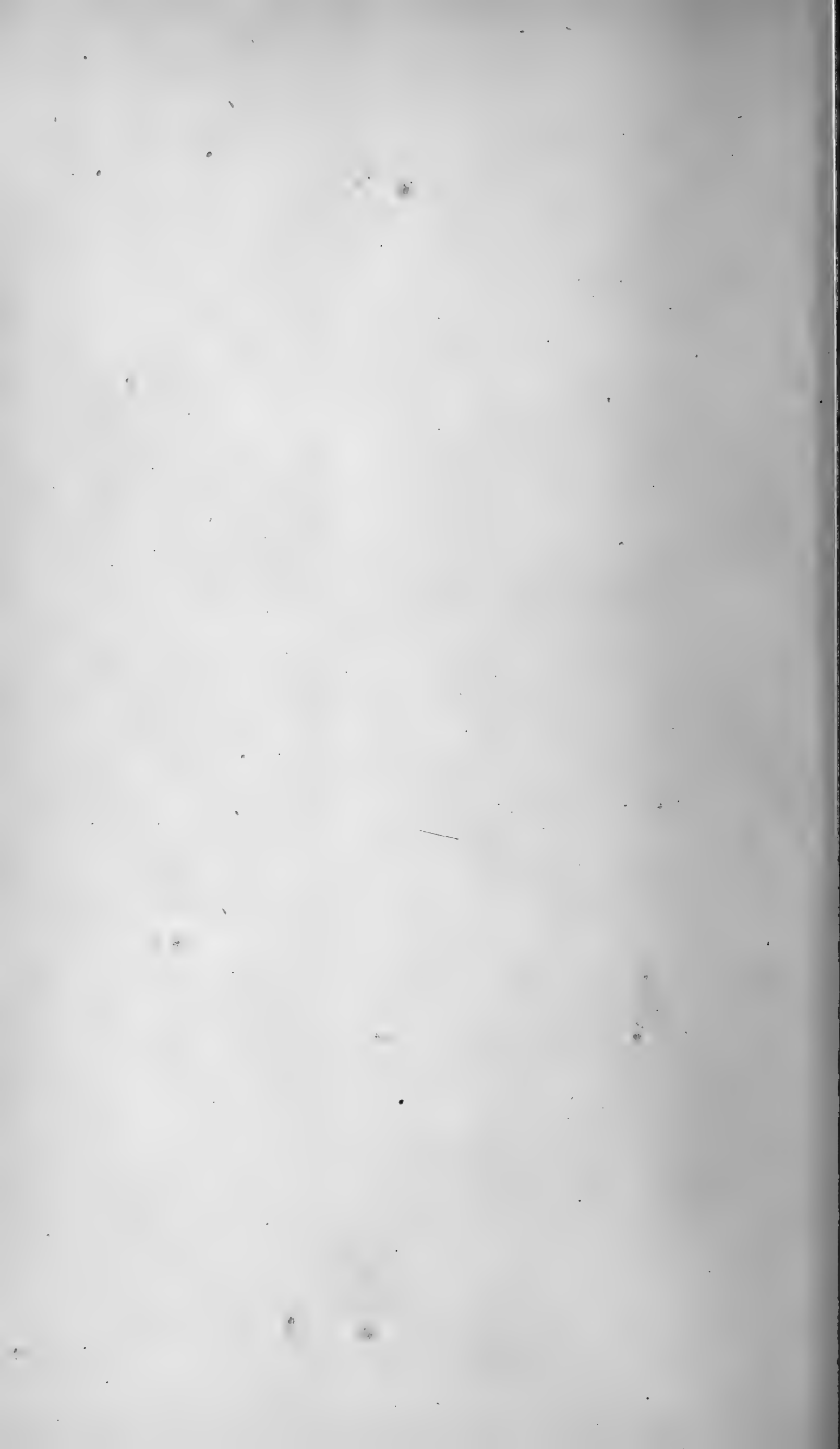


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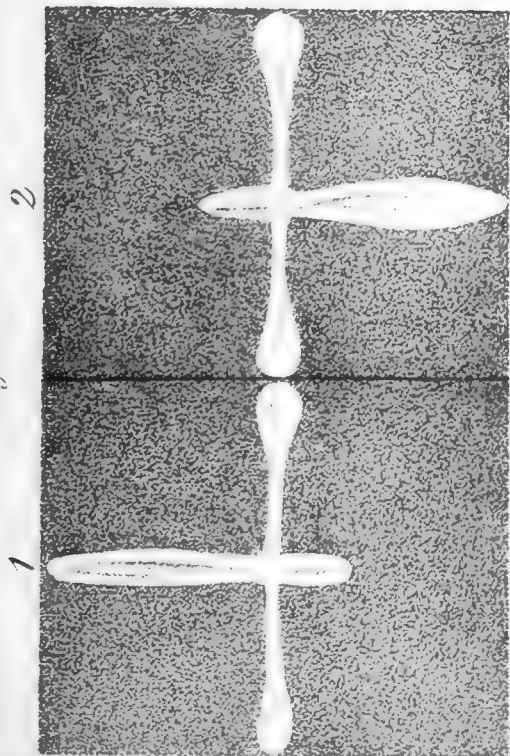


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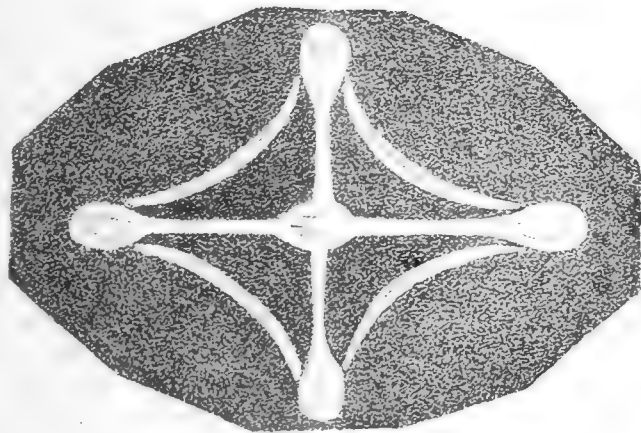


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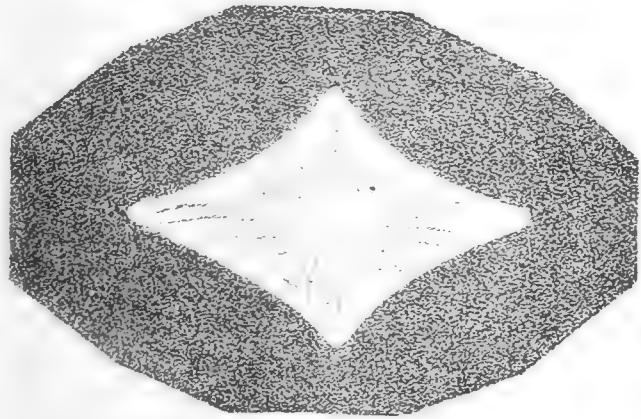


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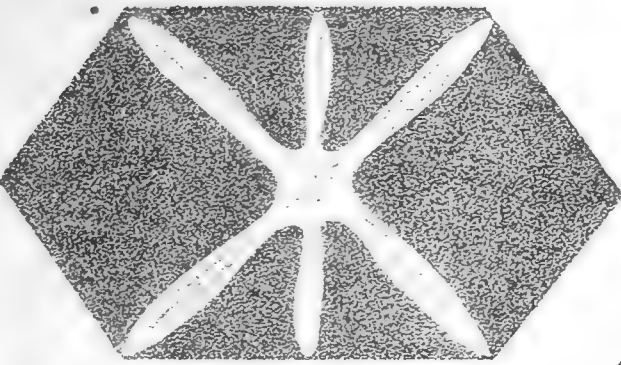


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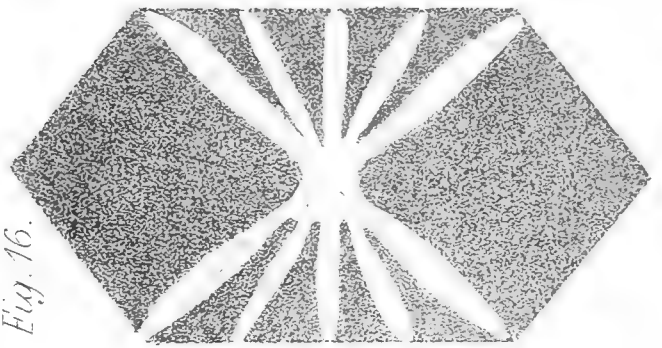


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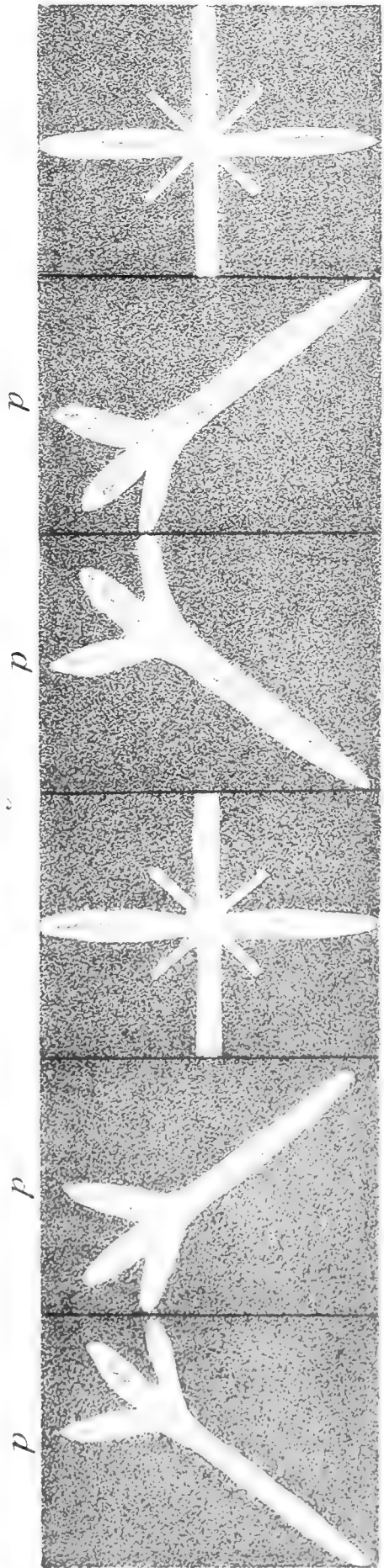


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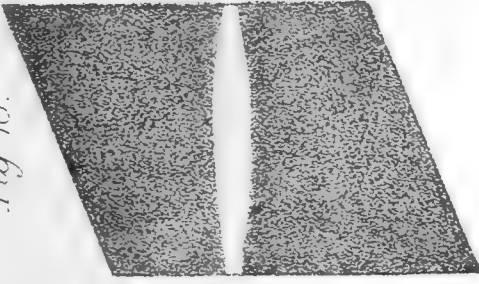


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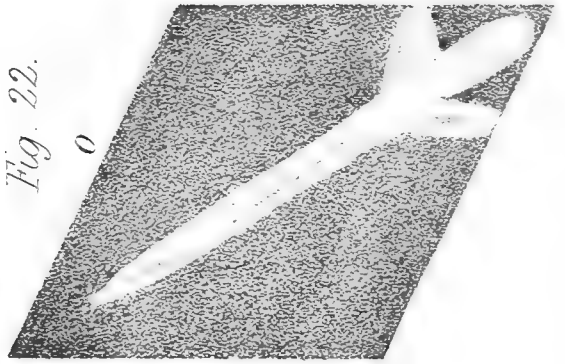


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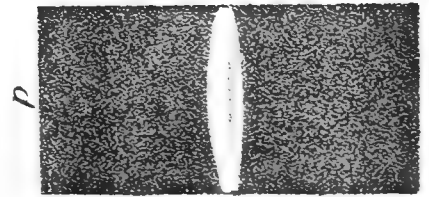


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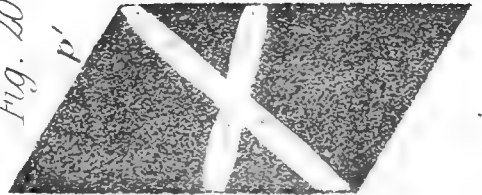


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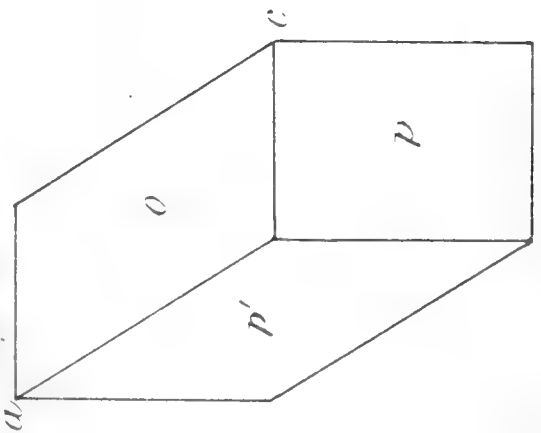
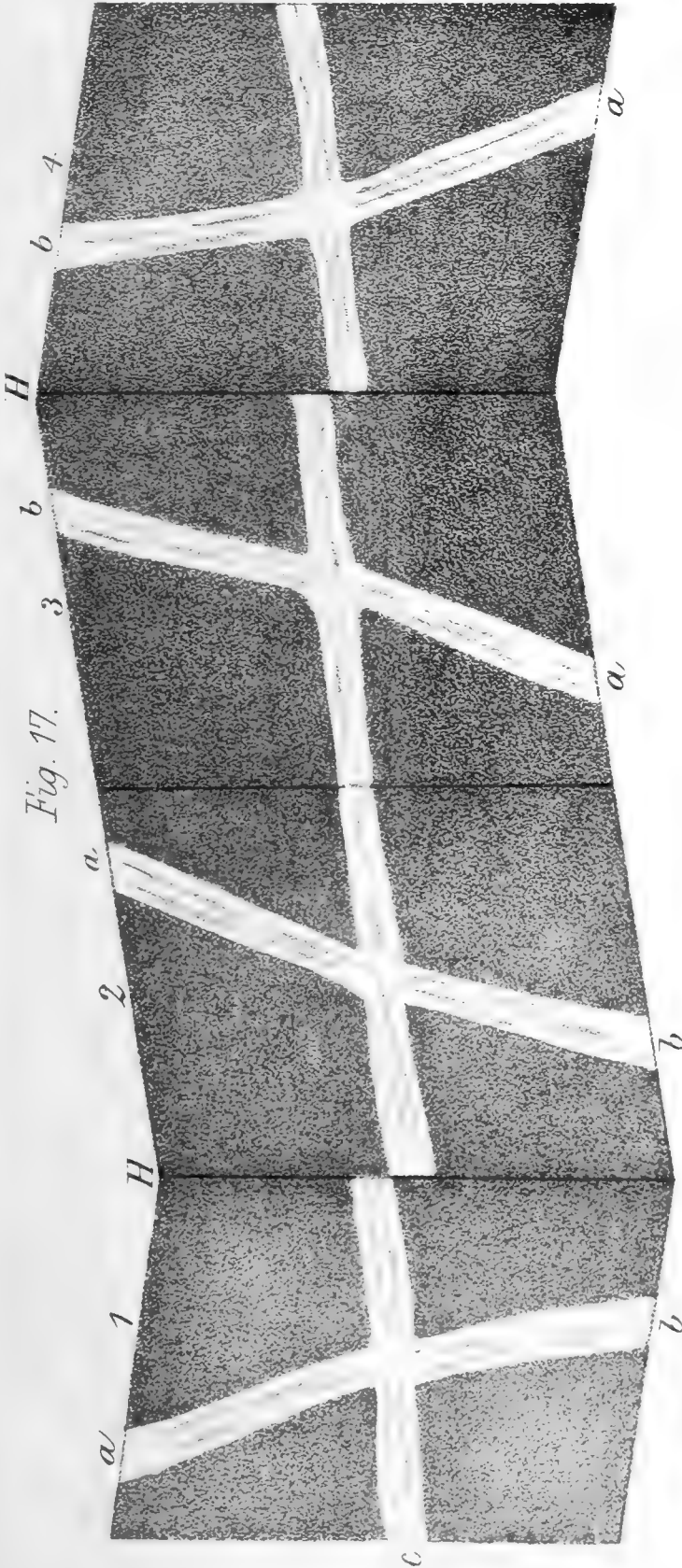
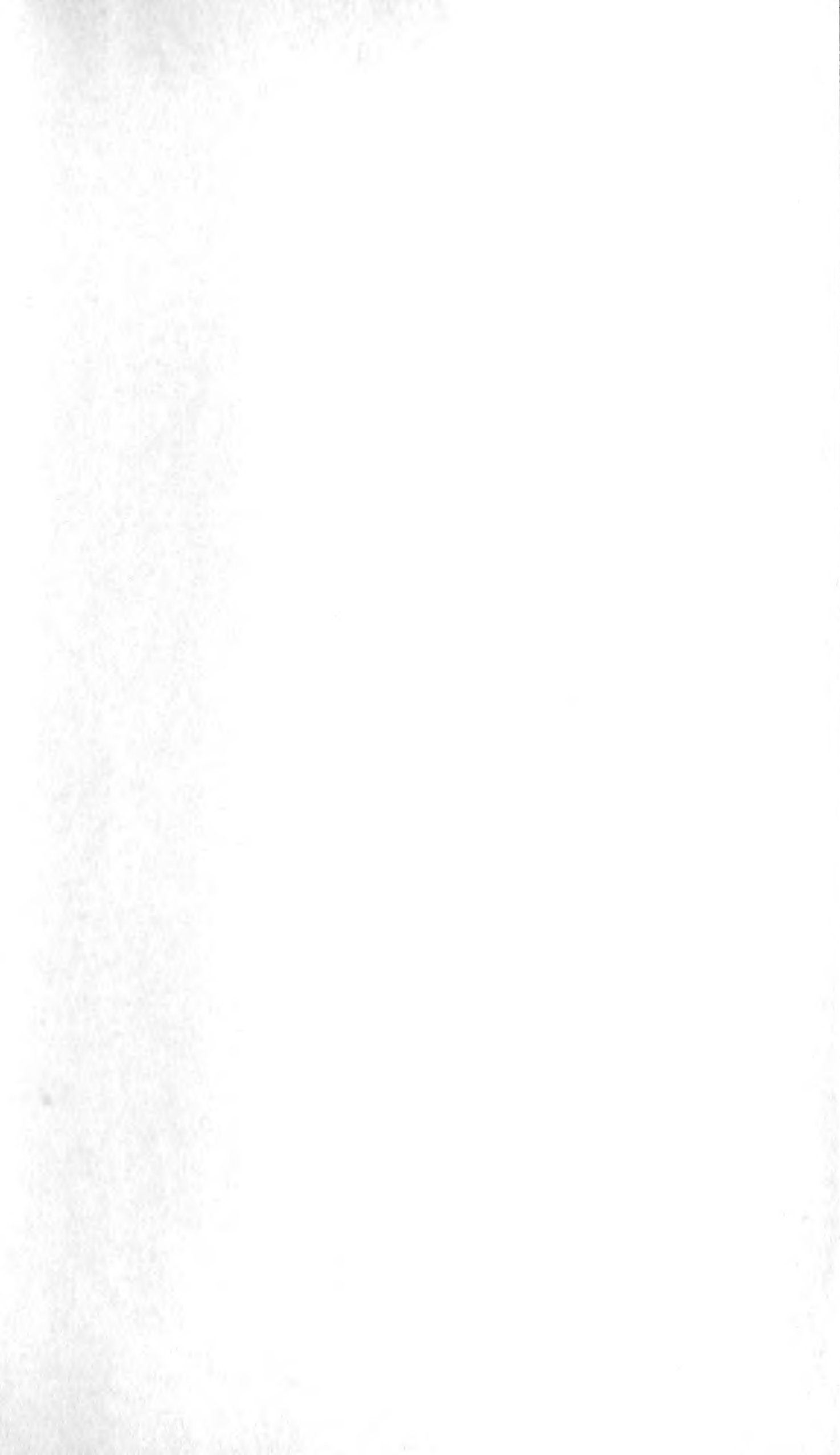


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